THE

PHILOSOPHICAL MAGAZINE
AND JOURNAL:

COMPREHENDING

THE VARIOUS BRANCHES OF SCIENCE,

THE LIBERAL AND FINE ARTS,

GEOLOGY,

AGRICULTURE,

MANUFACTURES AND COMMERCE.

BY ALEXANDER TILLOCH,
M.R.I.A. F.S.A. EDIN. AND PERTH, &c.

"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Monit. Polit.* lib. i. cap. 1.

VOL. XLV.

For JANUARY, FEBRUARY, MARCH, APRIL, MAY, and JUNE, 1815.

LONDON:
PRINTED BY RICHARD AND ARTHUR TAYLOR, SHOE LANE:
And sold by CADELL and DAVIES; LONGMAN, HURST REES, ORME, and BROWN; MURRAY; HIGHLEY; SHERWOOD and Co.; HARDING; UNDERWOOD, London: CONSTABLE and Co. Edinburgh: BRASH and REID, and NIVEN, Glasgow: & GILBERT & HODGES, Dublin.

THE

PHILOSOPHICAL MAGAZINE
AND JOURNAL:

COMPREHENDING

THE VARIOUS BRANCHES OF SCIENCE,

THE LIBERAL AND FINE ARTS,

GEOLOGY,

AGRICULTURE,

MANUFACTURES AND COMMERCE.

BY ALEXANDER TILLOCH,
M.R.I.A. F.S.A. Edin. and Perth, &c.

"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." Just. Lips. Monit. Polit. lib. i. cap. 1.

VOL. XLV.

For JANUARY, FEBRUARY, MARCH, APRIL, MAY, and JUNE, 1815.

LONDON:
PRINTED BY RICHARD AND ARTHUR TAYLOR, SHOE LANE:
And sold by Cadell and Davies; Longman, Hurst, Rees, Orme, and Brown; Murray; Highley; Sherwood and Co.; Harding; Underwood, London; Constable and Co. Edinburgh; Brash and Reid, and Niven, Glasgow; & Gilbert & Hodges, Dublin.

CONTENTS

OF THE FORTY-FIFTH VOLUME.

CONTENTS.

CONTENTS.

CONTENTS.

THE

THE

PHILOSOPHICAL MAGAZINE AND JOURNAL.

I. *On the Nourishment produced to the Plant by its Leaves.* *By Mrs.* Agnes Ibbetson.

To Mr. Tilloch.

Sir,—I shall now continue the account begun in my last letter, tending to prove the different manner which nature pursues in distributing the nourishment the vegetable world requires in four different ways; viz. by a common root, by the agency of a bulb, by the cuticle of leaves, and by a sort of pump inserted into another plant. I have shown in evergreens, in firs, and in water plants, that no nourishment whatever is by them taken in by any other vehicle than the root, though some points and hairs may assist them in receiving those various juices that produce the bark mixture, and supply the oil to humectate and lubricate the spiral wire.

In trees, shrubs, herbaceous, annuals, and all those of the kind which prove luxuriant plants, I have given examples of those fed by the root and atmosphere; where the size of the plant, the quantity of their leaves, and the consequent evaporation and exhaustion are *such*, that they must require all the nutriment that can be bestowed *by both*. Here the root not only does its office *thoroughly*, but the leaves also, expanded and stimulated by light, are constantly receiving nourishment from the bounteous dews, as well as a variety of liquids and gases from the hairs and retorts: yet so admirably is the exterior managed to guard them from the too copious evaporation, that every single leaf is covered by many skins, two or three of which being impervious to water, allow air alone to exude. Thus all that passes out from the leaves is converted into oxygen for the benefit of man and animals, and only assumes its watery form when it escapes confinement. This circumstance alone might prove to those

who cannot or will not take the trouble to make use of their eyes, that all vegetables must be covered with an *impervious skin;* since without this no such quantity of oxygen gas could be retained within the cuticle; and without this protection most leaves would evaporate all their moisture in one single hot day. In the grassy leaves of early spring flowers, I gave an example of those leaves which are wholly supported and fed by their interior matter laid up for them in their bulbous roots, and requiring from the time of being taken out of the ground neither *earth, water,* or *rain,* (at least very little of the latter,) till after flowering they are replaced in the *earth* to receive and form the *seeds* and flowers of the *next year;* as bulbs will not continue bearing or forming seed or flower except they are replaced in the earth to *renew the seeds;* while the rock plants, wholly different, are fed by the cuticles of their leaves alone, and deprived of most of the impervious ones which in all other plants shut out moisture; these receive rain and dew from a variety of pores with which the excrescences on their leaves are *filled,* and which form nourishment enough *even to sustain very large plants.*

Having now given a sketch of the preceding letter, I shall turn to the subject of the present; which will describe the manner in which *sand plants* are fed; also how the parasite tribe receive their *nourishment;* nor shall I leave out those leaves which flower in the leaf, as most admirably drawing the line between those *parts* requisite to form the flower, or those adapted to the leaf only. I shall then give the description of the cuticle of wet plants, and finish by detailing the changes operated on plants from soil and situation, moisture and dryness, with a few other matters appertaining to the subject.

The real sand plant very frequently possesses a black, dry, and *shrivelled root,* which sends up but little *nourishment* except at its first shooting: when it is an annual, it is often discovered with its root half decayed, as if having conveyed the seeds to this their proper destination, at the summit of the plant: the root was no longer wanted, *but* to *yield* that trifling degree of sap required to support the stem; all its other juices and nourishment proceeding from the cuticle of the leaf.

The leaves of sand plants have three cuticles above and two below; two of each being impervious to water or any liquid. The pabulum, scarcely thicker than the bark juices, is filled with a glutinous matter, which however hardens as the age of the leaves increases. But of all the vegetable tribe no plants possess so many hairs as the sand plants: sometimes they are laid horizontally in layers both *above* and *below* the leaves, as at Plate 1, fig. 1. the *chenopodium olidum;* sometimes standing perpendicularly as in the turnip leaf, fig. 2, but in that case it has always

ways a shining cuticle above the impervious one, into which points or pockets are so contrived as to *receive* the *moisture from the atmosphere*: see fig. 3, 4. It is a shining net very frequently taken for perspiration, indeed it is continually filling with water, but instead of *giving it out* it is *taking it in.* The quantity of water or different liquids received by means of the cuticles of this species of sand plant is excessive; but they not only draw nourishment from the hairs and the cuticle, but the side of each leaf is a reservoir for accumulating nutriment round the plant: see fig. 5, 6. In the turnip they are a sort of retort, filling and emptying into the vegetable every three or four hours; fig. 6, *full; empty* 7. I often find them all empty, on generally one side of the leaf at a time; and *these* when thus situated plainly show how the *hairs* are *formed:* see fig. 8, 9. The glazed matter with which they are made, appears to me exactly to resemble the impervious skin (so often mentioned) at the *exterior* of *each face* of the *leaf*. The *texture is the same,* and it is also exactly like the shining skin which forms the cuticle of the sand plant I have just described; which, whether full or empty, makes so shining and brilliant an appearance in the microscope, that without taking it off it cannot be ascertained whether it is *full* or not of *moisture;* but the hairs, as soon as they empty themselves into the plant, grow as flat as their valves will let them. When vacant, they often draw up like a cork-screw, if they have many valves; but if only two, one at each end, they hang like a wet rag till refilled. I have found this species of gauze both *striped* and *spotted,* and in some plants it is *so thick* as wholly to disguise the colour of the liquid it contains. But wherever seen, and *in whatever plant found,* it is the *same matter,* with trifling variations, always impervious to water, and, though so very slight, capable of bearing even *detonation*—which I have often seen two liquids, on meeting, produce in the retort while under my eye. These innumerable hairs are the principal marks of a sand plant.

To give a perfect idea of the means used in nourishing a parasite plant, I need only describe the method by which innumerable loads of small muscles fasten themselves so forcibly on the rocks upon our coast. The muscle has a broad round part, *hollow* in the *middle;* the shell is laid on the rock with a gelatinous matter which fills up all the interstices of its scollop *except the middle cavity,* which is full of air: this the fish either exhausts or draws inward for its own use and support, thus leaving a vacuum, which fastens down the shell more *powerfully* than any glutinous liquid could do. In the same manner is the pump of the parasite plant composed; it has a *broad piece* which is held down by means of a vacuum, managed on the nurse plant:

 but

but besides this it has a hollow pointed vessel or instrument which only fills up *the very centre* of the *middle*, the rest between the centre and broad piece remaining a perfect vacuum. This point runs deep into the supporting plant, drawing from it a perpetual stream; and thus possessing itself of all the nutriment required, while the vacuum surrounding it is perpetually maintained by the superior force of the larger *vegetable*. I have examined the pointed part inserted into the mother plant (see fig. 9) at different *degrees* of *depth;* and in the dodder tribe it is so very *clear* that its insertion is most easily to be *perceived*, though entering far into the plant, especially if the furze is *young* and *tender*. This description will, I fear, appear rather obscure; but the drawings, see fig. 8, 9 and 10, will I hope elucidate it.

There are a great variety among the parasite plants: some, *like the ivy*, are *half fed* by the root, half by the nurse plant to which they cling. Some, like the *orobanche*, fix their root *on the nurse plant* that is to feed it. But though it seems to draw all its nourishment from the plant to which it adheres: yet much of its support is gained from the earth, on which its large and spreading roots repose: it has many fixed appendages growing from the root, which appear to me to play the office of a *sponge*, and suck up much moisture, which it undoubtedly yields to the plant, since *all the vessels of communication* with the real root appear. But there is a peculiar circumstance belonging to this plant which should not be overlooked: it possesses such a quantity of *tannin*, that my hands were absolutely *dyed* for a long time after dissecting it. I am well assured that the quantity of tannin is not always proved by the excess or depth of the colouring matter, and that there may be much of the *latter*, with *little of the former*, as in the alder: but I believe when it is *fixed and difficult to eradicate*, and that it has a *glazed appearance*, the *quantity of tannin* always predominates. The *orobanches* (especially in a sandy soil) grow in *such quantities*, and their roots are *so large*, that to collect them might be no bad speculation, since there is often a pound of root to each small plant.

The dodder tribe presents another variation of the parasite. It soon loses its root, and depends on the joint aid of the nurse plant and atmosphere; its pump is nearly the same as that in the ivy, but it is only a small piece of the stem that draws moisture from the atmosphere. The flower itself, however, never closes after it has once opened, and constantly exposes its most curious pointed nectary (it should seem) for this purpose, since it is always loaded with diminutive specks of water on the projecting points. Thus there are various means of nourishment made use of by nature in this species of plants: some gain from the

the *root* and *nurse plant,* some from the atmosphere and nurse plant. There are however others, that, not satisfied with drawing from the exterior or bark of the tree, dive deep *into it,* and, running a large vessel from one pump to another, lay a sort of claim on the wood, that must be highly prejudicial to their nurse. Of this kind is the *viscum,* and many West India climbers; which will often bind the wood as cruelly within, as the ivy does at the exterior of the plant. There is besides a peculiarity belonging to most parasite plants that deserves notice; they will often show, by some change of form, how much support they gain or lose from their nurse. Thus the ivy alters its leaves the moment the pumps are no longer suspended on the side of the tree, because then the greatest quantity of food comes from *the root,* and the juices of that part are no longer balanced by those which it received from its pumps. The *orobanche* draws much from the atmosphere the last few weeks of its existence: the taller, therefore, it grows, the more unlike itself it often becomes; and I found three last year, whose upper leaves were almost *herbaceous,* and had lost all that stringy woven appearance the *orobanches* are so famous for. But the most curious alteration I ever experienced in a parasite plant was that produced in an *ophrys nidus avis,* which had fixed its principal roots on the *roots* of a *birch,* and was covered some inches above the ground with dead leaves, to which its quantity of small sponges adhered. It exceeds the *orobanches* in these appendages, but they are always above the ground, instead of under; but they were not as in that plant *loose,* one side adhered to birch and one to oak leaves, and each side of the plant not only differed in colour, but the scaly sheath was of a different *texture.* I was never so happy as to find the plant but once, and I hope next year to be more fortunate.

All the pumps of the parasite plants proceed from and are formed in the wood only, and may be traced directly to it; therefore the juices are evidently drawn in to supply the place of sap: another convincing proof that the sap runs only in the wood. But when its pumps proceed as far as the wood to acquire its juices from the nurse plant, then indeed it does very great damage, and this is always the case in the *viscums;* it then soon spreads the rot through the body of its supporter. As to the formation of the leaves of the parasite plant, this generally depends on the circumstance, whether the leaves *do or do not* receive moisture *from the atmosphere.* If they do not, as in the *ivy,* their leaves are then formed like the evergreen; if *they do,* they are formed like the sheaths of the *orobanche.*

I must now mention a species of plants which I did not at

first consider, but which, from the extraordinary difference of the formation of their leaves, well deserves a place here. I have long before shown, that plants flowering in the leaf teach more than any other vegetable I am acquainted with, as drawing an *exact division* between *those ingredients* wanted *to form* the *flower*, and those merely required *for the leaf only*. It was the thorough study of these plants that first taught me the complete distinction between them, which has been of more use to *me* than almost any *dissection I could name*, as drawing a very exact and precise line *never infringed* by nature, and enabling me to avoid that confusion *perpetually* to be observed in many authors in describing the flower and the leaf. The leaf alone, as I have shown before, is formed of *much bark;* a very trifling degree of wood; a pabulum composed of the bark juices; quantities of spiral wire; and much of that impervious skin *often described*, and which prevents the leaf's *too great evaporation:* but when the flower is to be placed within it, then *the line of life is added*, a great deal more wood, and the pabulum decreased; and in its place the *flowers* and *seeds* fill up the interior: see fig. 12. This sort of leaf *is always stiffer and harder*, as having such a quantity of wood to convey the sap to the flower; and it proves this to be its *purpose*, by the manner in which the *sap vessels* lie. In the *xylophylla* (which flowers in the *contour of the leaf*) all the sap vessels run from the midrib to the edge, with an open mouth at the bud. In the *ruscus* (which flowers in the middle of the leaf) the sap vessels, after *running* in the shape of a heart, arrange themselves in the *centre* and stop at the middle point, while in the fern (where the leaf flowers in the direction of the spires) the sap passes on to the line of the flower, and there stops. This description may appear trifling; but a minute knowledge of the formation of every part of the vegetable, clear and without confusion, can alone enable us to profit by every *accidental fact* nature presents us with, and to which perpetual watching makes me a witness. I have seen so much confusion arise from endeavouring to account for *facts* without this *acquirement*, that I shall never but rejoice that it was the very first object of my *search;* nor can I ever think it can be too *minutely exact*, or too *perfect*, since I am convinced that that knowledge can alone serve as a foundation for the more useful studies of gardening and agriculture. It is the first step of the ladder. I cannot leave this species of plant without mentioning how completely they prove that subject I have so *much at heart*, that "all vegetables form the *heart or essence of the seed in the root*." In the fern in particular, the seeds are to be *traced* not only *all up the stem*, but through the

the *midrib of the leaf*, in a manner *too plain* to be *denied:* before the flower has at all appeared at the exterior of the leaf, it may be seen under the skin in knots, and down the midrib of each leaf; and if that is divided, they may be taken, and, if placed in the microscope, show the seeds and baskets. It is still more evident in the *xylophylla.*

I now turn to the cuticle of water plants. I showed in a former letter, that there are two sorts of water plants, those which lie on the surface, and those which rest below it: sometimes they are joined in the same plant, as in the water lilies. It is the latter sort which rise each day to the top of the water to fill themselves with air, nor could they carry on their interior mechanism but for this resource. The water *ranunculus* and many of the *potamogetons*, if watched, will be seen, though often loaded and twined together into a real weight, to rise by degrees with a sort of undulatory motion, till the whole has appeared for a few seconds above the surface of the water. I have taken them in the very act of rising, and found them almost void of air: but when, after giving them a little time, I have allowed the part to rise, and sink again, and then taken them quickly from the water, I have always found them loaded with air, one large bubble being constantly attached to each bud, and absolutely necessary to its existence; for, if taken away by pressure, the bud directly decays. I have long been convinced that air is as indispensable to vegetable as to animal life. All the trials that have been made to oblige plants to grow in an exhausted receiver *prove this*, but do not try the experiment half so well as pressing those bubbles of air out of water plants, because the vacuum is not to be maintained perfect enough or sufficiently long for a plant to shoot. But in order to be assured whether the buds could live under water if each was robbed of its bubble of air, I took some *ranunculus aquaticus* to grow in a deep tub; then seized a piece about an inch or two; and after pressing away the bubble, I tied it at each end, so that the air could not return; but the bubble would not leave the bud without *breaking the skin* which *contained* the air, so completely was it linked to that which it was to support. I replaced the piece directly in water, but the buds were dead and the line of life black in less than an hour. It is inconceivable how soon a fresh-water plant dies, if taken from its element; quite as quickly as a fish, and decays much sooner, for it grows putrid almost directly. May it not be the same thing that kills it, which kills the fish? viz. the want of inward air to sustain the excessive weight of the outward atmosphere. There is undoubtedly a very strong similitude in their interior formation. But what brings on that immediate *decomposition?* There is certainly a very curious

fact

fact respecting the putridity of one part of a vegetable, well worth the attention of a *good chemist*. When a plant is gathered or cut down, the line of life rarely lives a few hours, but the gatherer of the leaf grows black sooner, and even precedes it in decomposition. Behold that of the French bean, or *phascolus vulgaris;* the *acacia*; plane tree: indeed *any leaf* whose gatherer is large enough to show it well, and it will be found to begin its decomposition almost within the hour. I used to fancy it was the quantity of oil taken in by the hairs to lubricate the spiral wire; but it has so much resemblance to the general decay of the water plant, that I think I must be mistaken; a fresh-water plant soon decays in salt water, and a sea-weed soon decomposes in fresh water. We know that a fish carries down a large bladder of air to support it, and so does the fresh-water plant; but there must be some very curious reason for its decomposing so *quickly*. I have read (but I cannot recollect my author) that the fresh-water plants at the side of the rivers in Africa lie down *like the sea-weed,* and recover again when the rainy season restores the waters: I think it must be a mistake, for then they must be wholly free from all *interior vessels,* as are the salt-water plants.

I shall now endeavour to show the curious changes produced on the cuticle of leaves, by the *excess* of *humidity* or *dryness*. When a semi-water plant loses the rill or ditch that *sustained its moisture,* I have before shown that its air-vessels by degrees contract till they scarce deserve the name, gaining also additional quantities of hairs on their leaves, to make them some little amends for the moisture their root used to dispense. Thus they continue to grow like land plants, as long as the ditch remains dry that used to feed them.

Viewing a quantity of *circæa lusitanica,* I observed that those which grew in or near the stream had (as usual) their stems and leaves full of air vessels, and that they were almost wholly free from hairs; but on examining some higher up the bank, they were not only entirely free from air-vessels but had also quantities of hairs to supply them with water, and at the summit of the bank their upper cuticle was crippled with points. Now this is a sort of succedaneum I have so frequently *discovered,* when *dissecting plants,* that I believe it is the common resource of nature either in very *dry* or *very wet weather*. I have often on these occasions (when wet weather has long prevailed) taken up roots, and found them (especially in herbaceous plants) partly covered at the exterior with a thin layer of air vessels, as if to protect them from the too great humidity of the ground; and when, on the contrary, nature has been oppressed with a superabundance of dry weather, I have found the roots equally covered with

with hairs of innumerable sorts; both nearly a temporary expedient.

Thus far Nature assists; but I have in vain sought for some cure, some *indication of change,* to remedy to a plant the *alteration of soil.* I cannot find that she adopts any plan to enable a *sand plant* to endure a clay, or a chalk plant to support a sandy soil. These are therefore far more fatal transformations to vegetable life, than any humidity or dryness can produce. I have repeatedly *traced 'isorders* (which appeared to me to arise entirely from that source) that afterwards tainted the *very means of life* in a plant. But how bountiful has Nature been to us in this respect! leaving however to our own industry the care of culling each different species, and adapting it to the right soil. There is scarcely a plant necessary to the food of man or beast, that has not so great a variety in its species, as to enable the industrious *grower* to choose that which best suits his own ground. *Corn* of every kind, *vetches, clovers, grasses,* have all their different sorts properly fitting the soil from which they first came, and in which they originally and spontaneously grew. Thus there is a clay clover, and a sand clover, a chalk clover, one suited to a poor land, and one that grows well in rich land only; one that will not support moisture, and one that grows only in wet land; one that prefers hills, and one that can only do well in valleys; one that likes the sun, and one that avoids it. Nature has been equally bountiful in most other plants peculiarly adapted to agriculture; and in wheats also, of the many I have tried in *different soils,* I have found them remarkably well answering this plan, and regularly attaching themselves to that sort of ground from which *each came,* and *especially belonged:* and, in wheats it is of the utmost consequence to discover those which suit the soil, and, having found two or three, change only from one to the other when the alteration of seed becomes necessary. Custom is of astonishing use in plants; and though the ground, for an excellent reason, should not continue too long receiving one plant, yet it always grows better for being accustomed to the soil. What is meant by growing sick of the ground, is, that it exhausts after a time that peculiar juice which is necessary to the plant; but give it a season or two cessation, and it will always grow better for being used to its situation.

I have been showing how much plants, especially leaves, *gain* by the *atmosphere.* Let me now revert to the use it is of to the *ground* itself, which it may truly be said to manure, and, being done by nature, has a much greater effect than any dressing we can bestow on it. Much has been said lately against the extravagance and bad husbandry of *fallows:* surely this is not just.

The

The ground must be manured by some means. No earth has as much *salt* and *oil* as is required for a proper cultivation; this must be *added.*—It is hardly possible to conceive the gain of a *winter fallow*, if the ground is well opened to receive it; but then it must be *free* from weeds, that they may not rob the soil of that benefit it would otherwise receive; and it should be turned over frequently, that the *salt* and *juices* by lying too long above may *not evaporate.* Poor earths require even more than rich ones; as wanting it more, it has a *greater effect.*

Dr. Dickson, in his most excellent book on Agriculture, advances a proposition which I must (though with all humility) dissent from. He says, "the red clover affords a large produce of leaf and blossom, on which account the land is kept in a more perfect state of closeness and shade." This I do not deny; but I do the inference drawn from it, when he adds, "it has therefore much more influence in *ameliorating* and improving the soil, and affords a better preparation for wheat crops." What is it ameliorates the soil? the being shaded from the atmosphere? This is so exactly the contrary, that that *very circumstance* makes it a more exhausting crop: if *shading* from the atmosphere would be sufficient to better the ground, then a fallow must impoverish it. The earth, the water, and the atmosphere bestow on each alternately nearly the same juices, but the atmosphere is the richest source of gain. If we cover the earth with a vegetable which receives all these bounteous dews, the plants alone profit by it, and the soil possesses not itself of any part. I think the idea must have arisen from some sand plant, such as the *turnip;* and the farmer finding that the ground was always *better,* rather than *worse*, for the crop (*though heavy*), and not knowing the difference that exists in the quantum of nourishment *some plants receive from their leaves,* concluded that every plant that equally shaded the ground must have the same effect. Now it is a sand plant *alone* that can do this, taking so little from the root, and of course from the soil. If it has very large leaves, it collects so great a quantity of juices from the atmosphere, so much more than is sufficient to form the pabulum of the leaves, and the bark juices, that, running down into the stem, it enters the root, and probably *dispenses* to the *earth* that *superabundance* of *nutriment* it could not *contain within.* But this is far from being the case in *other vegetables;* for all herbaceous plants, (which take more than two-thirds of their support from the root,) if they have large leaves, exhaust a ground greatly. Thus *rape* and *hemp* can never *precede corn* or any white crop; because they take still more from the ground; whereas there cannot be a better preparation for wheat than *turnips.* But I turn again to a fallow, or receiving that from the atmosphere into

into the ground, the cuticle of the sand plants would take; nor is it the least important gain in a fallow, that *much less dung is necessary*, since *nature herself manures the soil.* Calley is of opinion that dunging *naked fallows* is better dispensed with, and has often in tolerable loams made the crop to fail. This is almost the only instance I have been able to collect where too much manure has been deemed a defect. Sinclair gives also an example where potatoes were placed, drilled between three rows of fallow; and though the former was dunged much more than the latter, there was infinitely more wheat in the *naked fallow*. A winter fallow is an excellent thing in light ground, and as a preparation for spring wheat; but it will not do with *clays*, which require a thorough drying and pulverizing, before they can profit by the falling juices, which would only render the earth more hard and compact. A summer one is therefore more proper for this soil.

The idea of letting a crop of any kind grow, in order afterwards to plough it in, and benefit by what it has received from the atmosphere, and the rich juices it may possess, is (if I may humbly offer my opinion) the worst way of enriching the ground I ever heard of. The crop is lost; the juices, being quite fresh, are not in a proper state (too crude) for being turned into the ground with any good effect, and what it received from the atmosphere has already been absorbed up into the plant. Would not the same time *laid down* in a *naked fallow* do ten times the service?

I have now endeavoured to begin to apply some of those studies that have employed so many years of my life, to that *occupation* I am so very desirous of *benefiting*: I mean *farming*. I have tried here to show how necessary it is a farmer should be well acquainted with the nature of his ground, the soil and subsoil of *his estate*, and with the various plants he cultivates. They are but few in number; still he should know whether they are *dry* or *moist plants*, that he may not reverse their situations; that he may also know whether the plant is a *sand, rock, herbaceous*, or *parasite plant*, that he may deal out his manure *accordingly*; nor give it in quantities to *that plant* which wants but little, but dress it in proportion to the quantum of juices it receives from the earth, and place it in its proper position for accepting those of the heavens; turning it also to the aspect it best likes. It is particularly of consequence in wheat, that we may not, by trying it in every soil, and thus growing it frequently in improper ground, load it with disorders that spread far and wide, and will at last make the seed degenerate to half its size, and destroy the very life of the plant. But that farmers, after a thorough trial, may keep to the few species that suit their *own soil*,

soil, nor be continually changing from caprice, or the hopes of greater gain; for, if any thing can make a poor ground yield as much return of profit as a rich soil, it is the putting in those plants that are natural to it; and, by manuring, raise them *to* the greatest degree of perfection they are capable of in this country; and as we have every opportunity of exchanging with each other, it can little signify what individual vegetables we grow, provided they are excellent of their kind. Does it not agree with reason, that the plant must grow best in that soil from which it originally issued, if with that it receives those *juices* that will enrich it, which it requires, and which nature (as if to stimulate us to industry) has alone refused it? And when it is considered what money is exhausted in making plants grow in unnatural and improper ground, instead of searching for and putting in the plants suitable to the soil, the farmer will surely be convinced that he will save half the expense in manure and seed, by acting as I venture to advise*.

To prove how completely the plant is made for the soil, how it is made to accommodate itself to it, I shall give *in my next* a few examples of the *sand* and boggy plant, the *sand* and *herbaceous* plant, comparing *their roots*, and the manner with which they support the defects and weight of each peculiar earth; how completely the radicle and side roots point out those plants that are so nurtured by the earth, and how different are those which receive their support from the leaves; how in clay and chalks, the very cuticle of the roots is formed to bear the putrid water often found under the surface, and which is in general more disagreeable to plants than any other defect.

I must now mention a circumstance which I forgot to notice in my last letter, when describing the astonishing manner in which *bulbs* will remain without *earth*, water, or air, while they *flower* and *seed*. But on looking back to my book of observations, I find *that on trying*, they would never either flower or seed the *second year* without being *placed for a time in the earth*, when their seeds immediately formed, and their flowers began to swell; and I had marked it as *another proof* that the seeds proceeded *from the root*—if fresh evidence is required to substantiate a fact, to *those* whose eyes cannot view so small an object.

I am, sir, your obliged servant,

Sherwood, near Newton St. Cures. AGNES IBBETSON.

* Whoever may wish to see the continuation of the agricultural part of this letter, may probably find it with the name of the plant fitted to each soil, in the Bath Agricultural Review.

De-

Description of the Drawing.

Plate I. fig. 1, 1, 1, 2, 3, different sand plants, showing how they appear when a breadth of the leaf is cut.

Fig. 2, being the upright hairs, while the figure 1s show the horizontal ones.

Fig. 3, shows the sort of borders that covers each face of the leaf and is often seen with the upright hairs.

Fig. 5, is a leaf which is too small in proportion, but is designed to show that one side of the leaf is generally filled and emptied at a time, as here represented, only that to show them well the hairs are drawn too large.

Fig. 6 and 7, the hairs showing full and empty.

Fig. 8, an upright hair, various figures of this sort.

In Parasite Plants.

Fig. 9, the manner in which the pumps are formed: some have three or four points in one case; some have only one; but they have all the loose part *a*, *a*, which falls over the point, and glues itself to the plant it is designed to live on :—a piece of the ivy, fig. 10.

Fig. 12, a breadth of the leaf as cut from the xylophylla plant.

II. *On the Rules for Algebraic Multiplication.* *By Sir* H. C. Englefield, *Bart. F.R.A. and L.S.*

To Mr. Tilloch.

Sir,—The following had been several years ago composed, but it is only a few months since the approbation it received from some friends well qualified to appretiate it induced me to send it to you for publication, if you should judge it worthy. Since it was written I have seen the work of Maria Agnesi, the celebrated female professor of mathematics at Bologna. She has in the part relative to subtraction made use of a reasoning similar to that adopted by me; but she has not extended it to multiplication. A friend pointed out to me the work of Mr. Woodhouse, entitled Principles of Analytical Calculation.

In this excellent work, peculiarly valuable for the clearness and precision with which it is written, I found with great pleasure that the mode of treating the subject of multiplication was in effect similar to my own, though expressed with more conciseness, and perhaps not so evident to mere beginners. I therefore have still thought that what I now send you may be of use to that class of readers, and in that hope transmit it to you. I am, sir,

Your obedient servant,

Tilney Street, Nov. 1, 1814. H. C. Englefield.

NUMBERS are abstract representations of quantity, and have no particular meaning till applied to some object. Thus 10 may mean ten feet, ten apples, ten pulsations, ten distances of the sun from the earth; but they have one clear and distinct idea attached to them, that the unit, whatever it be, is taken ten times. Every sum is therefore in fact a multiple of unity; and in common multiplication, the multiplicand being considered as unity, the primary and fundamental idea is the same as in the simplest figure or figures written down.

But in pure symbolic algebra the abstract is carried much further; for the letters are not only, as in numbers, general signs of quantities, but do not even represent any definite numbers of unities; and therefore are rather receipts or directions for the performance of operations, than real operations themselves: just as a man who has written a perfect receipt for making a pudding cannot be said to have made a pudding, but told how that pudding has been or may be made.

$a+b$ only means, that when instead of those letters some definite quantities are used, they are to be added together, not that they are now incorporated; and to call this by the name of addition, seems an abuse of terms, or rather a confusion of definitions. In like manner $a-b$ means that b, being less than a, is to be taken from it when by the substitution of some real quantity this can be done. And this is so true, that, if improper or dissimilar quantities are used, the expression remaining the same, absolute nonsense ensues.

If, for example, a be supposed to represent ten shillings, and b seven miles, it is obvious that nothing but absolute nonsense is talked when we speak of $a-b$.

In what has been called geometrical multiplication, the abstraction, though very analogous to that of algebraic notation, is not however precisely the same. In this a superficies is considered, having its four angles right angles, and its sides the two given lines which are thus considered as multiplied by, or drawn into, each other; and the results of this operation are rigorously true, and may be as logically reasoned on when the values of them are inexpressible by numbers, or are incommensurable, as when they are commensurable. The properties, for example, of the area formed by the side of a square and its diagonal may be investigated, though no numbers can express them. In this respect the abstraction is very similar to that of pure algebraic symbols, which may be supposed to represent any quantities of the same kind, whether expressible by numbers or not. In another point of view the abstraction of geometrical investigation is very similar to algebraic. The figure drawn is not the

the real representation of that under consideration in the mind: it is only a sort of assistance to the mental process. The lines though crooked, the angles though not right angles in the figure drawn, represent straight lines and right angles, to the reasoner; and still further, the figure which he thus contemplates has no determinate magnitude to his mind, and indeed cannot have any; for, if it had, the deductions would not be universal, but particular. It would, I believe, be of great advantage to the young student to consider these matters attentively on his first outset, as many false ideas would be thereby prevented from entering his mind.

Perhaps too much has been said on this even for a learner:—to proceed to the common rules of what is called algebraic multiplication.

1st. $+ \times +$ is $+$plus, $a \times b$ evidently means that a is to be taken b times. $a + b \times c$, that the sum of a and b is to be taken c times: but as a and b cannot in this state be really added together, we can only say that when they can, a is to be taken c times, and b is to be taken c times, and the mode of writing this direction shortly is $ca + cb$.

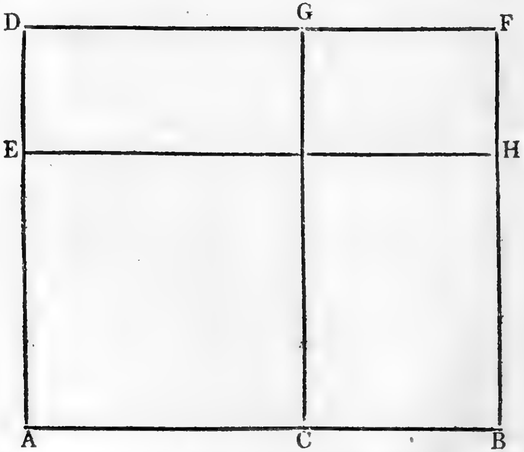
2d. That $+ \times -$ is minus, admits of an equally clear demonstration or rather explanation. We must first remember that no quantity simply considered can be $-$ minus. It must be compared with some other quantity either greater than itself or of an opposite direction. In the common operations of algebra it is only used in the former sense, and $a - b$ merely means that b being less than a, it is when it can be done, to be subtracted from a; $a - b$ therefore really means the difference between a and b: if then $a - b$ is to be multiplied by c, having first taken a, c times, we have evidently done too much, for that would be the product of a only, taken c times; what then is to be subtracted from ca in order to have a true result? evidently b taken c times, cb, and the direction will be, multiply a by c, and from the product take the multiple of b by c. That is in the short-hand of algebra $ca - cb$: nor can this operation be announced in this mode of directing our operations in any other than this circuitous way; but in practice the operator would certainly go a shorter way to work, and having first subtracted b from a, would multiply the remainder by c.

3dly. $- \times -$ produces $+$.

This most perplexing rule to every clear-headed student, and which has been rendered only more obscure by every attempt I have yet met with to explain it, (for demonstrations I cannot call them) may be equally explained by following one step further the above train of reasoning. If $a - b$ is to be multi-

plied by $c-d$, or taken $c-d$ times, the evident meaning is, that the difference of a and b is to be multiplied by the difference of c and d. Now we have before shown that if $a-b$, be taken c times, the direction for this operation is $ca-cb$. But at present we are to have a less result, as the multiplier c is diminished by d. Something more therefore is to be taken from ca, than in case 2. Now as we can only operate on the symbols separately, we will now take from ca, dg, and the expression will be $ca-cb-da$. But it is obvious that we have now taken away too much from ca, for da is the whole quantity a taken d times; whereas the quantity we ought to have taken away was only a lessened by b, d times. It is therefore obvious that having in the expression $ca-cb-da$, lessened ca too much by db, or be taken d times, we must add to that expression db, and the true direction will be $ca-cb-da+bd$: and as these symbols are of no particular meaning the rule must be universal, and must extend to multinomials as well as binomials, for in all cases the reasoning is precisely the same.

This may also be demonstrated geometrically with great care.



Let the line AB$=a$, BC$=b$, AD$=c$ and DE$=d$. Then will AC be $=a-b$, and EA$=c-d$. Complete the parallelogram ABDF and draw CG and EH through C and E parallel with DA and AB.

Then the rectangle AF will represent ca, the rectangle CF, cb, the rectangle EF, da, the rectangle GH db, and the rectangle CE,

CE, the result arising from multiplying $a-b$ by $c-d$. Now in the formula $ca-cb-da+db$ we are directed first to take from AF; CF, and then EF, but the rectangle GH being common to both CF and EF, has been already taken away when we took away CF, and as in the algebraical formula this cannot be told, we take away the whole rectangle EF, and then add the rectangle GH, which if it had been taken out of the rectangle CE would evidently have left the result of the operation too small.

III. *New Outlines of Chemical Philosophy.* *By* Ez. Walker, *Esq. of Lynn, Norfolk.*

[Continued from vol. xliv. p. 441.]

The composition of the atmosphere is still but imperfectly understood. It is said that it consists of 21 per cent. of oxygen gas and 79 of nitrogen; but it is well known that hydrogen and carburetted hydrogen gases arise from various species of vegetable and animal decay and putrefaction, and in great abundance from marshes and stagnant pools of water, especially in hot weather. Consequently those gases would soon contaminate the atmosphere to such a degree, as would render a country uninhabitable, were it not for the natural means by which those gases are decomposed and returned to the earth. Hence it is probable that the upper parts of the atmosphere generally contain a large portion of those gases.

The air upon the tops of mountains is much colder than in the valleys, because hydrogen gas, being much lighter than atmospheric air, rises in great abundance into the higher regions; but oxygen gas cannot ascend so high, in consequence of its being many times heavier than the other.

Hence, the temperature of the air in the higher regions cannot be equal to its temperature upon the surface of the earth where those gases are in a more condensed state*.

"In every case of combustion there must be present a combustible and a supporter;" nor can the temperature of a body be increased without these two elements be united.

To illustrate this position by experiment; Let two pieces of dry wood be smartly rubbed together, and an increase of temperature will be generated long before combustion takes place†. Consequently *combustion and increase of temperature are effects of the same cause.*

* Phil. Mag. vol. xliii. p. 103. † Ibid. p. 103.

But

But this theory does not rest merely upon the hydrogen gas, which rises from the surface of the earth into the atmosphere, to supply it with one of the elements of combustion; for it has lately been proved that nitrogen is a compound of oxygen and hydrogen.

Decomposition of Azotic Gas or Nitrogen.

Since the departure of Professor Berzelius from England, he has written to a chemical friend in London, announcing that he has succeeded in decomposing nitrogen, which he has reason to suppose, from his analysis, to be composed of about 45 parts of a new species of inflammable gas, and about 55 of oxygen*.

And Mr. Mires, a young chemist in London, has proved, by a series of very ingenious experiments, that azote is a compound of oxygen and hydrogen, nearly in the proportion of those announced by Professor Berzelius †.

Now let it be supposed that 100 parts of atmospheric air are composed, by weight, of 21 parts of oxygen gas, and
79 of azotic gas.
100

And if the azote be composed of 45 per cent. of hydrogen and 55 per cent. of oxygen, then 79 parts of azote contains
35·55 of hydrogen, and
43·45 of oxygen.
79·00

For, as 100 : 79 : : 45 : 35·55 hydrogen.

And, as 100 : 79 : : 55 : 43·45 oxygen.

Consequently if the hydrogen gas contained in the nitrogen be taken into the account, 100 parts of atmospheric air contains 64·45 of oxygen = 21 + 43·45, and,
35·55 of hydrogen.
100·00 ‡.

Whence it appears that the atmosphere always contains the two elements of combustion, and these being acted upon by the influence of the sun, the temperature of the air is increased §; and as his influence increases or decreases, the temperature of bodies, in which these elements are united, varies accordingly. Hence we may infer that all the variations of temperature

* Monthly Mag. vol. xxxv. p. 571. † Annals of Phil. vol. iii. p. 364.

‡ Girtanner proposed the hypothesis that atmospheric air is a compound of oxygen and hydrogen, and that nitrogen gas obtained from it by the usual methods, he supposed to be formed by a portion of the oxygen being abstracted, and the remaining quantity entering into combination with hydrogen. § Phil. Mag. vol. xliv. p. 351.

which

which take place between the equator and the poles, the burning heat of the torrid zone, and the intense cold of the frigid, originate from one universal cause.

Lynn, Jan. 11, 1815. EZ. WALKER.

[To be continued.]

IV. *On the Star Polaris.* *By* GAVIN LOWE, *Esq.*

To Mr. Tilloch.

SIR,—THE annexed table contains the precession, abberation, nutation and solar variation, both in right ascension and declination of Polaris, for the year 1815. It has been computed with great care, and will serve with sufficient accuracy to the year 1820. The mean right ascension and declination of that star from 1815 to 1820, appear, from the best observations, to be as follows:

	H.	M.	S.		°	′	″
Jan. 1, 1815, mean Æ	0	55	50·22	Decl.	88	19	17·15
1816, ...	0	56	4·24			19	36·57
1817, ...	0	56	18·32			19	56·01
1818, ...	0	56	32·46			20	15·43
1819, ...	0	56	47·07			20	34·85
1820, ...	0	57	01·34			20	54·26

The general rule for finding the corrections that are to be applied to the mean right ascension and declination of the star, is, to enter the table with the sun's longitude, which is given for every day in the year in the Nautical Almanac, and take out the precession and aberration in the 2d and 4th columns of the table. In like manner with the longitude of the moon's node, had from the same Almanac, take out in columns 3 and 5 the nutation. Apply these corrections respectively to the star's mean right ascension and declination, and the apparent right ascension and declination are obtained.

Although the table gives the corrections only for every 10° of the sun's longitude or moon's node, it is easy to make proportion, and find them for each degree.

I am, sir,
Your most obedient servant,

Islington, Dec. 29, 1814. GAVIN LOWE.

A Table of the Precession, Aberration and Nutation of Polaris, for the Year the 1815, both in Right Ascension and Declination.

Argt. Longitude of ⊙ or ☊		In Right Ascension: Precession and Aberration and Solar Var.	In Right Ascension: Nutation.	In Declination: Precession and Aberration.	In Declination: Nutation and Solar Var.
s.	°	s.	s.	″	″
0	0	−39·31	−21·31	+8·44	+2·46
	10	40·16	21·84	12·27	+1·08
	20	39·74	21·74	+2·54	−0·37
1	0	37·79	20·97	−0·34	1·77
	10	34·48	19·56	3·02	3·10
	20	29·93	17·55	5·42	4·50
2	0	24·31	15·01	7·43	5·33
	10	17·82	12·02	8·99	6·14
	20	10·71	8·66	10·04	6·74
3	0	−3·14	5·04	10·53	7·11
	10	+4·49	−1·26	10·39	7·28
	20	12·26	+2·55	9·67	7·17
4	0	19·69	6·29	8·34	6·90
	10	26·66	9·84	6·40	6·43
	20	33·02	13·08	3·94	5·81
5	0	38·65	15·93	−1·03	5·05
	10	43·40	18·30	+2·33	4·17
	20	47·20	20·10	6·00	3·22
6	0	49·96	21·31	9·90	2·18
	10	51·71	21·84	7·13	−1·06
	20	52·16	21·74	17·94	+0·03
7	0	51·56	20·97	21·91	1·17
	10	49·81	19·56	25·66	2·30
	20	46·96	17·55	29·13	3·40
8	0	43·04	15·01	32·25	4·43
	10	38·17	12·02	34·85	5·36
	20	32·48	8·66	37·00	6·18
9	0	26·09	5·04	38·55	6·83
	10	+19·10	+1·26	39·50	7·26
	20	−2·03	−2·55	20·45	7·51
10	0	9·23	6·29	20·19	7·50
	10	18·48	9·84	19·35	7·23
	20	22·72	13·08	17·97	6·71
11	0	28·50	15·93	16·12	5·95
	10	33·30	18·30	13·84	4·95
	20	−36·97	−20·10	+11·25	+3·78

V. *On the combined Action of Water and Charcoal in oxidizing Metals.* *By* THOMAS GILL, *Esq.*

To Mr. Tilloch.

SIR,—A FACT stated by Dr. William Henry in the last Number of Thompson's Annals, of the oxiding of a metal (cast iron) by the combined action of charcoal and steam; brings to my mind a similar effect which occurred to our ingenious acquaintance Mr. T. T. Hawkins, several years since; who, in employing powdered charcoal placed between two perforated leaden plates, for the purpose of filtering water, found the water had become considerably impregnated with lead, which led him to take the filtre to pieces, when it appeared that the joint action of the water and charcoal had converted much of the lead into a white oxide, which interspersed with the charcoal, occupied the entire space between the plates, and therefore he was under the necessity of laying them aside, and substituting others in their places not liable to that defect; by which means he perfectly attained his object, that of employing charcoal as a filtering medium, for which, owing to its great porosity, it is admirably adapted. I am, with much respect, sir,

Your most obedient servant,
THOMAS GILL.

No. 83, St. James's Street,
London, Jan. 10, 1815.

VI. *Account of a Fall of* Uranolytes *(Aërolites) near Agen.* *By* M. DE SAINT AMANS*.

ON the 5th of September 1814, a few minutes before mid-day, the wind being northerly, and the sky perfectly serene, a violent detonation was heard in the communes of Montpezat, Temple, Castelmoron, and Montclar, situated in the first, second, and fourth *arrondissemens* of the department of the Lotand Garonne. This unusual detonation was immediately followed by three or four others at an interval of half a second successively; and finally, by a rolling noise at first resembling a discharge of musketry, afterwards the rolling of carriages, and finally, that of a large building falling down. These detonations, which took place towards the centre of the department, were heard with more or less intensity within a circle of several leagues. Thus at Agen, four leagues off, they were sufficiently strong to

* *Annales de Chimie,* tome xcii. p. 25. Oct. 1814.

alarm

alarm some persons, and the concussion of the air was such as to shake the doors and windows of certain houses; while at Paymirol, two leagues to the eastward of Agen, these effects were less sensible; and at Mezin, St. Macaire, Basas, and Condon, situated five or six kilometres from the focus of the explosion, it was heard in a very indistinct manner.

At the end of this phænomenon, which, considering the state of the atmosphere, could not be occasioned by any storm, we were led to expect a fall of these meteoric stones, which has always been preceded by similar detonations. We soon learned, in fact, that this fall, accompanied by a kind of lightning, had taken place in the communes above named. From the written and verbal reports which have reached us, the number and volume of these stones appears to have been considerable. Some were sent to the prefect who has communicated them to the minister of the interior: others were distributed among the curious in various parts of France, while many were picked up by the peasants and venerated as reliques. Two are mentioned as weighing eighteen pounds each. It seems that they were not found warm at the moment of their fall: the heaviest were sunk into a compact soil to the depth of eight or nine inches, and one of them rebounded three or four feet from the ground. It is added, that these stones fell obliquely, making an angle of from 65 to 70 degrees with the horizontal line; finally, that they diverged in their fall, affecting various directions in the different communes where they fell. Like all those which have come from similar meteors, they appeared to be fragments of more considerable masses, and are perfectly homogeneous. All the specimens of these stones which I saw, present no character to the eye which can make them be distinguished from those which I have hitherto had occasion to examine, or which I have in my cabinet: they merely seemed to be more friable and more porous than the latter. I have remarked in some fragments globulous bodies, similar to those which Mr. Howard found in a great quantity in the uranolites of Benares, and which are composed, according to him, of abundance of silex with a little oxide of iron. We observed also in the interior of those stones, that the pyrites which they contain are sometimes crystallized in a group. All of them are covered externally with a black crust of the thickness of a quarter of a line nearly, which announces the action of fire, as we see in all the stones of the same kind. Two of our correspondents inform us that one of them exhibits singular impressions at the surface, but it is necessary to verify this.

In fact, of all the peculiarities which the phænomenon presented, the most remarkable is the very simultaneous appearance

ance of a small cloud, which seems to have accompanied the meteor, and even to have preceded it a few seconds. This small white cloud, grayish in the centre, appeared to move with the greatest rapidity over the district where the meteor fell. In other parts, and particularly from the spot where I observed it, it seemed stationary before the explosion. It has been generally admitted that this small cloud had a roundish form. Scarcely was it perceived in the communes where the uranolites fell, when the explosion, accompanied by lightning, was heard. At the very instant the cloud appeared to be divided into three or four parts which were rapidly precipitated towards the ground, leaving behind them irizations of a blueish colour and the point of which was red. From the position which I occupied, it was seen directly in the north, inclining a little to the north-west. It seemed then to be immoveable; but at the moment of the detonation it seemed to advance very rapidly towards the south, forming two points which were prolonged in the sky, and which the peasants unanimously compared to long cords. After this sudden movement, the small clouds which had attained nearly my zenith, considerably diminished, stopped, became immoveable, and ended by being insensibly dissolved at the same place. It cannot be doubted, I think, that the instantaneous appearance of this cloud insulated in a sky absolutely deprived of all vapour, is not connected with the meteor. It has been observed under the same forms, nearly in every place where the detonation was heard, and its immoveability, notwithstanding the strong wind which then blew, proves that it must have been very high in the air. We cannot, I think, refrain from regarding it as the produce of the gases emanated from the stoney mass which, when heated by the friction which it underwent in traversing the atmosphere, allowed them to escape under the form of a condensed vapour. The nebulous appearance which resulted must have given rise to several optical illusions on the part of the spectators, who before the explosion had no interest in observing it. To those who were close to the place where it fell, it seemed to move with a great rapidity: to those who were like myself, four or five leagues towards the south it appeared stationary. In advancing directly opposite to the latter, it must in fact have appeared to them without motion, until the explosion made it assume another form, and until, as it approached their zenith, they must have perceived its progressive motion. This cloud must therefore have been the result of the gases developed in the bosom of the mass, which must have in the first place formed around a spherule of vapours, and which being more and more rarefied, as the mass approached the surface of the earth, must have caused its explosion.

To

To conclude: this explosion must have been effected, as I have already said, in a high region of the atmosphere, since the wind had not reached the small cloud, and since the fragments of the mass were dispersed, diverging over four communes in a radius of five great quarters of a league. If similar clouds have not always been remarked simultaneously with meteors of this kind, since they have been observed with care, this has arisen from few of those meteors having been seen in such a serene sky, and other clouds must have been confounded with the peculiar cloud which accompanies them.

Here let me direct the attention of my reader for a moment to the term *aërolite*, which is commonly given to meteoric stones. This denomination does not seem to be the best which may be employed. In fact, it is far from certain that these stones are formed in the air or with air. The elevation of the meteor which produces them having been observed to be at least thirty leagues from the surface of the ground, proves that they have nothing in common with the fluid which supports life on the surface of our globe. The name of *uranolyte* has long appeared to me to be better suited to bodies whose origin is unknown to us, but which tend towards the earth through that boundless space in which the stars move, and which is unanimously called the heavens. The term therefore which is formed of the Greek words οὐρανὸς and λίθος, deserves the preference to *aërolite*, as being more definite. Some writers have even adopted the term *uranolite*, since the publication of a Memoir which I read to the Society of Agriculture, Arts and Sciences of Agen, in 1808.

VII. *Calculations of the Intervals and Beats of the Sounds yielded by various Gases in the corrected Experiments of Messrs.* F. KIRBY, *and* A. MERRICK, *recorded in Mr.* NICHOLSON'S *Journal. By Mr.* JOHN FAREY, *Sen.*

To Mr. Tilloch.

SIR,—IN your xxxviith volume, page 3, you did me the favour to insert some extracts and particulars, of an interesting series of experiments, by Messrs. *F. Kirby* and *Arnold Merrick*, of Cirencester, extracted from Mr. Nicholson's Journal, on the different pitches or degrees of sound, yielded by the same organ-pipe, when successively blown by eight different kinds of gases, in a suitable apparatus. Since that period these gentlemen have repeated their experiments with an improved apparatus for producing the sounds, and with additional care; having also extended

tended the number of their gaseous bodies (or mixtures of such to seventeen) without however adopting in these repetitions, the hints which I gave at page 5, for *determining the pitch* in future experiment, *by the beats* made between the gaseous sound and those of other pipes, previously and carefully tuned to fixed Diatonic Notes, near enough to them, in each case, and blown constantly by atmospheric air, in the same manner as the standard C pipe in their experiments was blown.

I recommended this course to Messrs. K. and M. in order to avoid the great uncertainty, which *practical tuners* have found, in *unisons* even upon sounding bodies of the same kind, as strings and strings, pipes and pipes, &c. and much more so in tuning strings from pipes, or *vice versâ*, &c. It is for enabling future Experimenters to resort to this more accurate method than the monochord, that would doubtless remove, or much lessen, the very considerable *disagreements that still exist*, between the repetitions of these gentlemen's experiments, and between their average results, on the present and former occasion, that I now trouble you with, an extract of their general results (from Mr. Nicholson's Journal, vol. xxxiii. p. 171) in the two first columns of the following Table; only reversing the order, that I might place the most acute sounds towards the top, as the notes are placed on the music stave.

The third column shows the intervals in my notation reckoned each way from atmospheric air; the fourth column shows the excess or defect of these, compared with the nearest Note on the Rev. Henry Liston's *Euharmonic Organ* (which remains yet on view at Messrs. Flight and Robson's); the method of tuning which Notes *by means of perfect concords only*, I have fully explained in your xxxviith volume, p. 275, and xxxixth volume, p. 374 and 421. And the 6th and last column shows, the *Beats* in one second of time, and whether *flat* or *sharp*, that these mean results (as indicated by the monochord lengths in column 2) would make with Mr. Liston's Notes in the preceding column, respectively, when Tenor-clif C is yielded by the standard experiment pipe, blown by common atmospheric air, and excites just 240 complete vibrations therein, in one second of time.

TABLE

TABLE.

Aëriform Fluids experimented on.	Mean Lengths of sounding Wire.	Intervals above or below the Sound with atmospheric Air. Σ	f	m	Comparisons with the Rev. H. Liston's Organ Scale. Notes and differ. in Σ.	Beats in one sec.
Ether Vapour (highest)	·0965000	747·3967	15	65	d*—4·6033	2·9146b
Ditto - (mean) -	·1063283	661·8153	13	57	c* +2·8153	1·6110*
Carburett. Hydrogen, light	·1084167	644·5875	13	56	c'*—3·4125	1·9219b
Hydrogen - - -	·1110000	623·8865	12	54	c' + 0·8865	0·4864*
Sulphuretted Hydrogen -	·2060000	77·9720	1	7	C'*—5·1770	1.5359b
Ether Vapour and com. Air	·2187500	24·7983	1	2	C'*—11·2095	3·1428b
Azote (or Nitrogen) -	·2216250	13·3846	0	1	C' + 2·3846	0·6548*
COMMON AIR - -	·2250000	0	0	0	C	0
Carbonic and Hydrogen -	·2333333	32·0604	1	2	C'b+3.9475	1·0285*
Oxygen and Nitrogen -	·2335000	32·6935	1	2	C'b + 3·3114	0·8634*
Oxygen - - -	·2362857	43·1948	1	3	*B'* + 2·8131	0·7243*
Olefiant - - -	·2386125	51.8700	1	4	Cb— 4·8700	1·2472b
Chlorine and Olefiant -	·2630500	137·9076	3	12	Bbb+ 2·0921	0·4841*
Chlorine - - -	·2699167	160·7268	3	14	*A*+ 0·2732	9·0616*
Carbonic Acid - -	·2787333	189·0430	4	16	*A'*b—3·0430	0·6665b
Nitrous Oxide - -	·2814375	197·5907	4	17	*A*b— 0·5907	0·1280b

In comparing the above results in column 4, it will be observed, that the Notes therein fall in three different Octaves, marked with small and capital Roman and with capital Italic Letters, as d, C and *B*. That *Hydrogen* yields a sound, very near an Octave higher than that from *Azote*, differing less therefrom than $1\frac{1}{2}\Sigma$, and would perhaps prove exactly an Eighth, in a more exact comparison by means of the Beats:—that *Oxygen* is still nearer (differing less than $\frac{1}{2}\Sigma$) a major Semitone (S, see plate V. in vol. xxviii.) below *Azote*: and this last being extremely near (almost within $\frac{1}{4}\Sigma$) to a minor Third above *Chlorine*.—*Nitrous oxide* is very near a major Third below *common Air*, &c.

The above was written long ago, soon after the appearance of the Experiments in Mr. Nicholson's Journal, but wishing to revise the calculations, it was laid aside for others engagements until now. I am, sir,

Your obedient servant,

Westminster, Jan. 13, 1815. JOHN FAREY Sen.

VIII. *Bio-*

VIII. *Biographical Memoranda respecting* Edward Hussey Delaval, *Esq. F.R.S.*

To Mr. Tilloch.

Sir, — As you have occasionally given place in your valuable Magazine, to some accounts of men of genius and real merit, permit me through the same medium to pay a tribute of respect to the memory of the late Edward Hussey Delaval, Esq. of Parliament Place, Westminster, who died on the 14th of August last, aged 85 years, and was buried in Westminster Abbey.

This gentleman was of a very ancient and noble family, and the only surviving brother of the late John Hussey Lord Delaval, which title is now extinct.

Edward Hussey Delaval was Master of Arts and Fellow of Pembroke Hall, Cambridge; an excellent classical scholar, and well conversant in most languages both ancient and modern. Chemistry and experimental philosophy were his favourite pursuits. He was a great judge of music and the polite arts; and the completest set of musical glasses ever produced in England, were made under his direction many years ago, and communicated by him to Dr. Franklin.

On the 28th of June, 1764, he published an Account of the Effects of Lightning in St. Bride's Church, Fleet-street, which happened on the 18th of that month. It was addressed to B. Wilson, Esq. F.R.S. with explanatory plates, and was read at the Royal Society, of which Mr. Delaval had been elected a Member in December 1759.

On June 7, 1769, by desire of the Dean and Chapter of St. Paul's, a Report was delivered to the Royal Society from W. Watson, B. Franklin, B. Wilson, and E. Delaval, on the means of securing St. Paul's Church from Lightning; and measures were in consequence taken for that purpose.

On the 22d of March 1772, St. Paul's Church was struck with lightning, it was examined by Mr. Delaval about a week afterwards, and the effect stated to the Royal Society.

Soon after this period a difference of opinion arose amongst the philosophers who had paid attention to electricity relative to the choice of pointed or blunt conductors for the safety of public buildings; and on February 20, 1773, Mr. Delaval in a letter to Mr. Wilson, published his observations on the subject, and gave the preference to the use of blunt conductors; stating, that the intent of conductors is to guard the building from danger rather than to solicit it; and adding the following remark

on

on the subject. "But although our conductors are inadequate to so great a power as that which is necessary to exhaust clouds of an immense size, yet they may be applied with great probability of success to carry off with safety as much lightning as may at any given time be contained in the metallic parts of a building, provided a complete communication be formed between them and sufficiently large conductors."

Impressed with a high opinion of Sir Isaac Newton's discoveries in optics, and by a close attention to those experiments which Sir Isaac Newton made, and has detailed in his Book of Optics. Mr. Delaval showed by a series of experiments, and minute observations that Sir Isaac Newton's doctrine is equally applicable to permanently coloured bodies; and on January 24, 1765, a Letter addressed by Mr. Delaval to the Earl of Morton, President of the Royal Society, was read there, containing Experiments and Observations on the Agreement between the specific Gravities of the several metals, and their Colours when united to Glass, as well as those of their other Preparations. This paper is published in the fifty-fifth volume of the Royal Society's Transactions, and for it Mr. Delaval was honoured with their gold medal.

In 1775, Mr. Delaval and Mr. B. Wilson were engaged in a Series of Experiments relative to phosphoric and the prismatic Colours they are found to exhibit in the Dark. The account was published by Mr. Wilson, who stated that the experiments were made in Mr. Delaval's house on the side of the Thames at Westminster.

In 1777, Mr. Delaval published in quarto, An experimental Inquiry into the Cause of the Changes in opaque and coloured Bodies, with an historical Preface relative to the parts of philosophy therein examined, and to the several arts and manufactures dependant on them.

On the 19th of May 1784, Mr. Delaval produced his experimental Inquiry into the Cause of the permanent Colours of opaque Bodies, which was presented and read at the Literary and Philosophical Society of Manchester, and honoured with their gold medal. It is published in the second volume of the second edition of their Memoirs.

In testimony of the great merit of Mr. Delaval's discoveries and observations, I adduce the approbation they have generally experienced amongst men of science, and which is evidenced by their having been translated into most of the European languages. I beg leave to quote the following instance, transcribed from the *Nouveaux Memoires de l'Academie Royal des Sciences et Belles Lettres,* noted by M. Le Professeur de Castellon, respecting

specting Mr. Delaval's "Recherches experimentales sur le Cause des Changements de Couleur dans les Corps opaques naturellement Colorées." Le Professeur Castellon thus observes, "C'est donc à l'expérience qu'il faut recourir si l'on veut connoître les causes des phénomènes de physique. C'est le méthode dont Bacon a si bien tracé la thèorie et dont Galileo et Newton ont si bien montré la pratique. C'est le méthode que M. Delaval, Membre de la Société Royal de Londrès, et mon confrère a cet egard, a exactement suivie dans le Memoire qu'il m'a fait l'honneur de m'adresser afin que je présentasse à cette illustre assemblée. Je le crois digne de son attention, et je vais le commettre a son jugement; j'ajouterai seulement que j'ai répété avec le succès le plus heureux plusieurs expériences continues dans cet écrit."

Mr. Delaval's company was much courted by persons of genius; and besides the other gentlemen whose names have been already mentioned, he was intimate with Dr. Lewis, Dr. Ingenhouz, Mr. Magellan, Mr. Kirwan, Mr. Cavallo, and most of the men of science of his time; though he visited little, his house was always open to men of abilities. He was ever ready to give information and elucidate it by facts in their presence, and in the accuracy of his experiments no man was his superior. No arts were made use of by him to trumpet forth his publications, but they were left to stand or fall by the test of experiment. The Royal Societies of Upsal and Gottingen, the Institute of Bologna, and the Literary and Philosophical Society of Manchester, unsolicited inrolled him amongst their members.

It may be necessary to note that Dr. Bancroft, in his publications intitled, Experimental Reserches concerning the Philosophy of Colours, has differed greatly in opinion with Sir Isaac Newton, and Mr. Delaval. Dr. Bancroft, in page 27, of the first edition of his work, makes the following remark, after long criticisms on Mr. Delaval's publications. "I also think it may be deemed a matter of excuse and consolation that he has only erred with Newton." It seems extraordinary after this assertion, that Dr. Bancroft, in his late publication, in two volumes, octavo, has so puzzled the subject, that in the Monthly Review enlarged, for September last, it appears that the Reviewers have concluded that Sir Isaac Newton and Mr. Delaval differed in opinion. This is not the first time that Dr. Bancroft has been unfortunate in his explanations, as may be seen in Dr. Robert Jackson's Letters to the Commissioners of Military Inquiry, page 5, where Dr. Jackson, in his Answer, saith; "He (Dr. Bancroft) has attacked my writings, and in doing this he has so mixed and garbled my words, so perplexed and perverted my meaning

meaning on different occasions, that without reference to my own text, I should scarcely know myself what I mean.

If Mr. Delaval had lived, I think he would have made precisely the same observation on Dr. Bancroft's attack upon his Treatise on Colours. I am of opinion, that if Dr. Bancroft would in a future edition of his work, give a detail of many important improvements made within the last thirty years or more in the practice of dying and printing, which seem to have escaped his notice, or of which he has not yet acquired a knowledge, he would render more service to the public than by his theories.

As Dr. Bancroft in his publications has commented very freely on the Theories and Opinions of Sir Humphry Davy, Sir Isaac Newton, Mr. Delaval, and many other English and foreign philosophers, I hope my present observations will not be deemed any seeming incivility towards him, but be considered as the frank effusion of a zeal for truth, an earnest wish to benefit the public, and a justice due to Mr. Delaval.

To enter into minute accounts of Mr. Delaval's experiments and theory would trespass too far upon your useful pages; but it is certain that whoever will attentively peruse Mr. Delaval's publications above mentioned, will find a series of very correct experiments, and a fund of most valuable information on the subjects they treat upon, and be pleased with the modesty and candour with which they are related.

An uninterrupted intimacy of forty years which I enjoyed with Mr. Delaval, continued to furnish me till his decease, constantly with fresh proofs of his strength of mind and great abilities. The various brilliant artificial gems made by Mr. Delaval, the curious samples of his mode of abstracting the fluor from glass, which remain in the family, and the neat Gothic house in Parliament Place, in which he resided till his death, the interior of which is elegantly formed of artificial stone under his immediate direction, in order to be perfectly secure from fire, will long remain durable testimonies of his knowledge as a chemist, and of his taste as a gentleman.

I remain, with much respect and esteem,

Sir,

Your obedient servant,

CHARLES TAYLOR.

John Street, Adelphi,
January 5, 1815.

IX. *A Copy*

IX. *A Copy of the Experiments made at the Royal Observatory, Greenwich, by Mr.* FIRMINGER, *Astronomical Assistant to the late Rev. Dr.* MASKELYNE, *by the Request and under the Inspection of the late Sir* GEORGE SHUCKBURGH EVELYN, *Bart. with a view to establish a Standard of Weight and Measure, by determining the Length of the Seconds Pendulum*.*

This Column shows the Position of the two Hands upon their respective Circles.		Sir George Shuckburgh's Thermometer	Time per Transit Clock.			Thermometer in the Obser. at Greenwich.	Barometer in Inches.	Arc of Vibration.
			Long Pendulum, first Series.					
Lower Index.	Vib.	°	h.	m.	s.	°		
41	0	52·4	4	41	1·1	51·7		
42	0			42	1·1			
43	0			43	1·2		29·79	
44	0	51·8		44	1·1	51·0		
10	0	50·2	5	10	2·3	51·1		
11	0			11	2·5			
12	0			12	2·3			
13	0			13	2·3			
14	0			14	2·3	50·$\frac{1}{20}$		3° 23′
54	0		5	54	4·4			
55	0			55	4·4			
56	0			56	4·5			
57	0	50·4		57	4·5	50·	29·69	
31	0		6	31	6·0			
32	0			32	6·1			
33	0			33	6·1			
34	0			34	6·1			
35	0			35	6·1			
36	0			36	6·1			
37	0	50·2		37	6·2	49·8	29·675	
35	0		7	35	9·1			
36	0			36	9·1			
37	0			37	9·0			
38	0			38	9·0			
39	0			39	9·0			
41	0	49·4		41	9·05	48·9	29·635	

* A description and drawing of the clock by which these experiments were made, has been given in Nicholson's Journal.

Short Pendulum.

Position of the two Hands upon their respective Circles.		Sir George Shuckburgh's Thermometer.	Time per Transit Clock.			Thermometer in the Obser. at Greenwick.	Barometer in Inches.	Arc of Vibration.	
Lower Index.	Vib.	°	h.	m.	s.	°		°	′
33	10·1	50·4	10	10	0	49·3			
35	10·7			11	0				
39	12·0			12	0				
41	12·7			14	0				
43	13·1	49·2		15	0	49·0			
53	35·3			50	0				
55	36·0			51	0				
57	36·4			52	0				
59	37·0	48·4		53	0	48·0	29·615		
46	10		11	16	0				
48	10·4			17	0				
50	11·0			18	0				
52	11·6			19	0				
54	12·1	48·0		20	0	47·5	29·594	3	30
37	2·1		12	11	0			3	32
39	3·0			12	0				
41	3·9			13	0				
43	4·3			14	0				
45	5·0	47·8		15	0	47·1	29·580		
48	1·2		13	16	0			3	32
50	2·0			17	0				
52	2·8	47·2		18	0	47·	29·558		
54	3·3			19	0				
54	14·1		21	14	0				
56	15·0			15	0				
58	15·4			16	0				
0	16·1			17	0				
2	16·8			18	0				
4	17·2			19	0				
6	18·0			20	0			3	25

Short

Short Pendulum continued.

Position of the two Hands upon their respective Circles. Lower Index.	Vib.	Sir George Shuckburgh's Thermometer.	Time per Transit Clock. h.	m.	s.	Thermometer in the Obser. at Greenwich.	Barometer in Inches.	Arc of Vibration.
35	3·9		22	4	0			
37	4·2			5	0			
39	5·0			6	0			
41	5·5			7	0			
43	6·1			8	0			
45	7·0			9	0			
1	12			17	0			
3	12·3			18	0			
5	13·1			19	0			
7	17·7			20	0			
9	14·3	48°		21	0	47°·2	29·490	3° 23′
12	10·9		23	22	0			
14	11·4			23	0			
16	12·0			24	0			
18	12·6	48 $\frac{4}{5}$		25	0	47·4	29·490	
3	3·2		0	17	0			
5	4·0			18	0			
7	4·6			19	0			
9	5·3	48		20	0	47·2	29·515	3 26
11	6·0			21	0			
53	38		1	12	0			
55	38·6			13	0			
57	39·2			14	0			
59	40·0	48 $\frac{2}{5}$		15	0	47·2	29·510	3 21$\frac{1}{2}$

Second Series of Long Pendulum.

0	0		2	26	1·			
1	0			27	1·			
2	0			28	1·1	47·3	29·510	3 16
7	0		3	33	3·6			
8	0			34	3·8			
9	0			35	3·8			
10	0	48 $\frac{4}{5}$		36	3·9	48·5	29·505	3 20

Second

Second Series of Long Pendulum continued.

Position of the two Hands upon their respective Circles.		Sir George Shuckburgh's Thermometer	Time per Transit Clock.			Thermometer in the Obser at Greenwich	Barometer in Inches.	Arc of Vibration.
L. Ind.	Vib.		h.	m.	s.			
3	0		4	29	6·0			
4	0			30	6·5			
5	0			31	6·5			
6	0	49° 7		32	6·5	48°·6	29·51	3° 24′
3	0		5	29	8·4			
4	0			30	8·5			
5	0			31	8·6			
6	0	49·0		32	8·7	48·1		
7	0			33	8·6		29·52	3 25

Short Pendulum.

23	21·0		10	23	0			
25	21·0			24	0			
27	21·2			25	0			
29	21·7			26	0			
31	22·3			27	0			
33	23·3			28	0			
34	24·0	46·3		29	0	46·2	29·635	3 $27\frac{1}{2}$
18	14·4		11	20	0			
20	15·2			21	0			
22	16·0			22	0			
24	16·4	45 $\frac{2}{5}$		23	0	45·9	29·66	3 26
50	31·3			51	0			
59	31·8			52	0			
54	32·6			53	0			
56	33·3			54	0			
58	33·9	46 $\frac{1}{5}$		55	0	45·6	29·674	3 25

The adjusting ball to the pallets was here raised to $2\frac{176}{200}$ inches, or 1·59 higher than before; all the other parts remained the same, and the ball was not afterwards altered.

Short Pendulum continued.

Position of the two Hands upon their respective Circles.		Sir George Shuckburgh's Thermometer.	Time per Transit Clock.			Thermometer in the Obser. at Greenwich.	Barometer in Inches.	Arc of Vibration,
L. Ind.	Vib.		h.	m.	s			
49	26·0		12	50	0			
51	26·3			51	0			
53	27·0			52	0			° ′
55	27·4			53	0			3 4
57	28·2			54	0			
59	29·0			55	0			
1	29·5	45°·5		56	0	47°·7	29·69	
6	7·5		20	34	0			
8	8·3			35	0			
10	8·9			36	0			
12	9·3	43		37	0	42·3	29·775	3 5
46	38·3		21	24	0			
48	39·0			25	0			
50	39·7			26	0			
52	40·3			27	0			
54	41·	44		28	0	43	29·785	
Here the small weight was taken off, and the large one put on.								
59	0		22	30	0			
1	0·4			31	0			4 0
3	1·3			33	0			
5	2·0			33	0			
7	2·6	44		34	0	43	29·785	
14	41·0		23	37	0			
17	41·6			38	0			5 30
18	42·1			39	0			
20	43·0	45·2		40	0	45·2	44·	

After the clock had been set going about three sideral hours, with the long pendulum, during which time five comparisons were made with the transit clock of the Observatory; these comparisons being finished, the moveable point of suspension was lowered just sixty inches as measured on the scale affixed to the side of the clock, and two series of vibrations were tried of three

 hours

hours each; after which the clock was moved back again, exactly to its former place, and another series was tried with the long pendulum of three hours: after this series was completed, the clock and maintaining power were both taken away, and a series of vibrations were observed with the simple pendulum as it hung from the clamp, and it was found that in 61 minutes of sideral time, the pendulum made 2560·24 vibrations. After this series of the long pendulum one of the simple vibrations of the short pendulum was tried, for which purpose the point of suspension was lowered 60 inches; and in 32 sideral minutes it made 2708 vibrations, also in 72 sideral minutes as tried again it made 6094 vibrations, but the last number being a little doubtful, another trial was made, and in 62 minutes were made 5248 vibrations: a trial of the long pendulum was then repeated, which in 65 sideral minutes made 2728 simple vibrations.

After these trials were finished, a series of experiments were made by changing the quantity of maintaining power, and also by altering the height of the adjusting ball to the pendulum, as will be seen by an inspection of the columns. It is necessary that before each trial the adjusting ball should be so situated that the beater should vibrate in the same time as the pendulum; but I am not aware that this was strictly attended to in the above series; indeed, I believe the weight was not so proportioned as to give an accurate result in this respect, as I remember Sir George intended to repeat the series at some future period: but his death put a stop to all further research. The clock is now in the hands of the Hon. C. Jenkinson, in consequence of his marriage with the daughter of the late Sir George, and I have no doubt he would be happy to give to any gentleman or society of gentlemen, who may be desirous of prosecuting these valuable experiments, by which the length of the seconds pendulum might be determined to almost any degree of exactness. A series of such experiments made in different latitudes would determine better than any other means the figure of the earth; the clock will admit of considerable improvement, and proportional and invariable impulse may be easily given to each length of pendulum; also various lengths might be tried, besides these selected by Sir George, and given above.

X. *On the Phænomena of Electricity.* *By* J. Murray, *Esq. Lecturer on Chemistry.*

To Mr. Tilloch.

Sir, — Some of my experiments are recorded in the pages of your Journal, which go far to establish the existence of a *current* and

and *countercurrent* as exhibited by the discharge of an electric sphere. In reverting to these phænomena, I would be understood as referring particularly to the wires *deposited upon cards with intervening patches of tinfoil*, and the *opposing balls obscured with China ink.* I have now the pleasure to adduce an additional illustration on the same side. In this experiment I employed the electrical forceps with its insulated stand, described in a late number of your Journal, a very good substitute for an universal discharger. Here one of the joints of the instrument was attached to the outside of the charged electric, and the other connected with the insulated discharger. A ball made of the pith of elder (about half an inch diameter) was placed in opposition, in contact with each end of the platina wires, the distance between the points varying from two to four inches and an half; when the discharge was made the balls *were propelled in opposite directions*, and when modified, *they exchanged sides.*

The propulsion of a pith ball introduced in evidence of a current from the charged to the opposite side is thus proved to be inconclusive. If we except the divergence and convergence of radii from opposing electricities, the inclination of the flame of a candle is the only experiment which can give the least colour to the theory of a maximum and minimum electricity. As to the *unipolarity* (as it is called) of the flame from this source, it is a well sounding word, but void of meaning, considered with regard to the view it is meant to establish; for it can, if I apprehend aright, receive a fairer and more philosophical elucidation from considering the phænomena displayed in the Voltaic circle, viewed in connexion with the recent experiments of Mr. Brande. Here it may be said, that the flame generating carbonic acid gas, ought to be attracted by the charged surface (pos.); whereas it is the reverse—my opinion is decidedly against this view. So long as it is *flame*, it is merely *carbon* in the *act of dissolving in oxygen*, for such is its constitution. It is *not of an acid nature*, but *carbonous oxide*, for carbonic acid gas is *uninflammable*, though *charcoal* and *carbonic oxide* (a gaseous body) be *combustible.* It becomes *not carbonic acid gas* until *ejected beyond* the *sphere of flame*, when being subjected to a *further exposure* relative *to atmospheric air*, it becomes *acid* by abstracting *an additional portion of oxygenous gas.*

Light and electrical phænomena appear *analogous:* both are material. I know that this has been denied to the first of these, but the *momentum of light* as proved by the experiment of Mr. Mitchell, in that projected against a delicate plane of copper, from a reflecting speculum, strongly supports the opinion;

 and

and is not the action of light on the photometer a proof of the same kind? My friend Capel Lofft, Esq. judiciously remarks, in his correspondence with me, If light were not material, but the undulation of an elastic medium, then the smallest *aperture* admitting light into a darkened room, should illuminate the whole apartment equal with the greatest, as a spark ignites any quantity of gunpowder however long the train. That electrical phænomena are produced by a material agent, appears beyond a question, from the exhibition of its effects on our senses. If the *taste* is operated upon, it is singularly affected. A snapping noise and loud explosion are communicated to *the ear*. Light and colours, and its effects on masses of matter are the circumstances embraced by the *orb of vision*;—our *feeling* is sensible of its power. In the spark drawn from an insulated conductor, the aura electrica from acuminated rods, and the shock from accumulated electricity; and it is impossible to mistake its *peculiar odour*. There is an experiment which, I think, confirms this statement; connect the external coatings of two jars, and by forming a communication between the inside linings, a *charge* may be transferred at a single contact from a jar charged in the usual way to another, and the density in each will be as the ratio of the capacity. Those substances which become phosphorescent by solar light, are precisely those which are similarly illuminated by electricity; for this assurance, I am indebted to the polite communication of Mr. Skrimshire, of Wisbeach: an opportunity of repeating his experiments has not occurred to me. The circle of prismatic rings, afforded by the electric discharge directed against a polished metallic plate from a point; and the concentric coloured zones exhibited by decomposing electric light, by means of the prism, are appearances allied to the supposition of the identity. A view of their corresponding chemical effects will come in hereafter.

Light moves with a velocity which is truly great, reaching the orb inhabited by us, a distance from the sun of 95 millions of miles, in less than eight minutes of time. The velocity of the agency of electricity harmonizes with that of light. The vast circuit traversed by it in the experiments of Dr. Watson, was performed quick as the lapse of thought. Caloric is, on the contrary, slow in its movement, and silver, the best conductor among the metals, proves its tardy progress, and sound itself in its propagation by the elastic shells of air percurs space, only at the rate of 1142 feet in a second of time.

Light is sent off from the sun's disc in right lines, the boundaries of shadows and its phænomenon when intercepted, evidence this. The parallelism of solar and electric light is a striking circumstance of their analogy.

If

If you will grant me permission, I shall continue ı ɔr-vations, and now only remark generally, that I believe æ-nomena of electricity are produced by *one* agent suscep . of undergoing *two distinct modifications*, the one *directly o. osed to the other*, each *possessing different relations to matter, from the other.*

I am, with much respect, sir,
Your obliged obedient servant,
J. MURRAY.

Witham, Essex,
January 16, 1815.

XI. *Letter from M.* AMPERE *to Count* BERTHOLLET, *on the Determination of the Proportions in which Bodies are combined, according to the respective Number and Arrangement of the Molecules of which their integrant Molecules are composed**.

YOU have been long since informed that the important discovery of M. Gay Lussac, on the simple proportions observed between the volumes of a compounded gas, and those of the component gases, gave rise to the idea of a theory which explains not only the facts discovered by this eminent chemist, and the analogous facts observed since, but which may also be applied to the determination of the proportions of a great number of other compounds, which in ordinary circumstances do not affect the gaseous state.

The memoir in which I enter upon this theory in detail, is nearly finished; but as occupations of another description do not admit of my finishing it now, I hasten to comply with your wish of giving an abstract.

Consequences deduced from the theory of universal attraction considered as the cause of cohesion, and the facility with which the light traverses transparent bodies, have led natural philosophers to think that the latter molecules of bodies were held by the attractive and repulsive forces which are peculiar to them, at distances as it were infinitely great relative to the dimensions of those molecules.

Hence their forms, with which no direct observation can make us acquainted, have no more any influence on the phænomena presented by the bodies which are composed of them, and we must seek for the explanation of these phænomena in the manner in which these molecules arrange themselves with respect to each other, in order to form what I call a *particle*. According to this notion we ought to consider a particle as the assem-

* *Annales de Chimie*, tom. xc. p. 43.

blage

blage of a determinate number of molecules in a determinate situation, containing among them a space incomparably greater than the volume of molecules; and in order that this space may have three dimensions comparable between each other, a particle must consist of at least four molecules. In order to express the respective situation of the molecules in a particle, we must conceive by the centres of gravity of these molecules to which we may suppose them reduced, planes situated so as to leave on one and the same side all the molecules which are beyond each plane. Supposing that any molecule is not contained in the space comprized between these planes, this space will be a polyhedron, of which each molecule will occupy a summit, and it will be sufficient to name this polyhedron, in order to express the respective situation of the molecules of which a particle is composed. I shall give to this polyhedron the name of *representative form of the particle.*

Crystallized bodies being formed by the regular juxtaposition of particles, mechanical division will therein indicate planes parallel to the faces of this polyhedron; but it will be able to indicate others resulting from the various laws of decrement: besides, there is nothing to hinder the latter from being frequently more easily obtained than a part of the former; and hence mechanical division may rather furnish conjectures, and conjectures only, for the determination of the representative forms. There is another way of ascertaining these forms: *i. e.* to determine by the relation of the component parts of a body, the number of molecules in each particle of this body. For this purpose I set out on the supposition that in the case where bodies pass to the state of gas, their particles alone are separated and removed from each other by the expansive force of caloric to distances much greater than those which the forces of affinity and cohesion have an appreciable action, so that those distances depend only on the temperature and pressure which the gas supports, and at equal pressures and equal temperatures, the particles of all the gases simple or compound, are placed at the same distance from each other. The number of particles is on this supposition in proportion to the volume of the gases*. Whatever may be the theoretical reasons which seem to support it, it can only be considered as an hypothesis; but on comparing the consequences which necessarily result with the phænomena, or the properties which we observe; if it agrees with all the known results of experience, if we deduce consequences which are confirmed by ulterior experiments, it may acquire a degree

* I have learned since the drawing up of my paper, that M. Avogrado has made this last idea the groundwork of an inquiry into the proportions of the elements in chemical combinations.

gree of probability which will approach to what we call in physics *certainty*. Supposing it to be admitted, it will be sufficient to know the volumes in the state of gas of a compound body and of its components, in order to know how a particle of the compounded body contains particles or portions of a particle of the two components. Nitrous gas, containing for instance the half of its volume in oxygen and the half in azote, it follows that a particle of nitrous gas is formed by the union of half a particle of oxygen and half a particle of azote; the gas formed by the combination of chlore and of the oxide of carbon, containing volumes of these two gases which are equal to its own, one of its particles is formed by the junction of a particle of chlore, and a particle of oxide of carbon; water in vapour containing, according to the fine experiments of M. Gay Lussac, an equal volume of hydrogen, and the half of its volume in oxygen, one of its particles will be composed of an entire particle of hydrogen, and the half of a particle of oxygen: for the same reason a particle of the gaseous oxide of azote will contain an entire particle of azote, and the half of a particle of oxygen: finally, a volume of ammoniacal gas being composed of a half volume of azote, and a volume and a half of hydrogen, a particle of this gas will contain the half of a particle of azote, and a particle and a half of hydrogen.

If we admit as the most simple supposition, (a supposition which appears to me sufficiently justified by the harmony of the consequences which I have deduced from it, with the phænomena) that the particles of oxygen, of azote, and of hydrogen are composed of four molecules; we shall conclude, that those of the nitrous gas are also compounded of four molecules, two of oxygeu and two of azote; those of the gaseous oxide of azote of six molecules, four of azote and two of oxygen; those of the vapour of water of six molecules, four of hydrogen and two of oxygen; and those of ammoniacal gas of eight molecules, six of hydrogen, and two of azote.

The supposition that the particles of *chlore* are also compounded of four molecules, cannot agree with the phænomena presented by this gas in its various combinations: we are necessarily led to account for these phænomena, to admit eight molecules in each of its particles, and to suppose either that these molecules are of the same nature, or that the particles of chlore contain four molecules of oxygen, and four molecules of an unknown combustible body.

The first hypothesis simplifies so much the explanations which are about to follow, that it would be a sufficient reason to use it for that purpose alone, even if we did not regard it as the most probable.

[To be continued.]

XII. *Ob-*

XII. *Observations on a new System of Phrenology, or the Anatomy and Physiology of the Brain, of Drs.* Gall *and* Spurzheim.

There are two fundamental errors into which people endowed with different proportions of certain mental faculties commonly fall, whenever they consider any new doctrine—1st, that of believing it before they have examined the evidence in its favour; and, secondly, that of condemning it anterior to investigation. These two states of mind are equally injurious to the advancement of science: they are opposed to each other, but are equally hurtful to the progress of truth—just as the rage for novelty, as well at the outcry against innovation, are both prejudical to real improvement. New discoveries too suffer from a variety of other reasons—the jealousy of the selfish and covetous, who envy their contemporaries the merit of invention, and the bigotry of ignorant persons, who imagine that some mischief will arise to their favourite creeds, as well as the great mass of imbecility and ignorance which pervades mankind, are stumbling blocks in the path of science, which nothing but steadiness and perseverance will overcome. There is, too, another and a very rational motive for persons to feel at first a prejudice agaist a new science, namely, that there have been such a number of impostors in the world from time to time, and the public have been so often duped by pretenders to philosophy, that cautiousness in admitting any thing new seems the natural and necessary consequence. This latter consideration operated powerfully on myself when first I heard of the discoveries of Gall and Spurzheim respecting the functions of the organs of the brain; but I was determined to investigate for myself the facts whereon the apparently new and peculiar notions entertained by those anatomists were founded.

During my studies of comparative anatomy, and while dissecting different animals when a boy, I was much struck with the generic forms of the crania of animals, and have often felt surprised that among the many boasted comparative anatomists whose volumes fill our shelves, none had made out any relation between the brain and the character of animals: they described minutely enough the number of vertebræ—they named with precision the bones of limbs—they measured the intestines to feet and inches: others counted the articulations on the backs of insects, the rings of the caterpillar, and the legs of the scolopendræ; but no one made accurate researches into the most important of organs. The difficulty attending these investigations of the brain, the laziness of individuals, and, above all, that execrable plan of yielding to authority, whereby people cease

cease to inquire into that which some great name had pompously declared to be unattainable, are the reasons why this interesting science had not been more successfully studied before the present day. These ideas frequently passed in my mind, but I was unacquainted with any mode of investigating minutely the brain and nerves. Constant presentiment, however, that in them would be found the material principles of our actions and character, prepared me, as it were, for the reception of the facts which came in a loose sort of form from Germany several years ago. I had always considered too, that the nature of man was never properly studied, and that a host of prejudices had deterred philosophers from the task. There were several other facts which occurred to me, by which I anticipated, something like what Dr. Gall afterwards promulgated. These subjects became additionally interesting from a curious observation I had made, namely, that the exercise of particular faculties violently and continuedly appeared to produce sensations in particular parts of the head. The violent action into which any organ may be called, frequently produces, I am persuaded, a sensation in the part, as Dr. Spurzheim has recently shown in his lecture on the physiognomical expression of the organs of the mind, and which is a very interesting fact, as it tends to establish the locality of action of the different parts of the brain. From these circumstances, and from many curious facts I noticed in diseases, I was then just in a state to be interested by the discoveries of Gall; and the meeting with his learned colleague in London, and hearing so many of my own crude notions detailed in the scientific manner in which they were treated of by Spurzheim, constituted by far the most interesting period of my life.

I had scarcely begun the investigation of the anatomical part of their system, when I exclaimed, All I have hitherto known of the anatomy of the brain and nerves is mere child's play; and the demonstrations of it given in our schools is merely dwelling on a blank in the science, and reminding us of the futility of inquiry!

As I consider this science not merely with reference to the interest it must excite as a means of knowing the principles of the diversification of the human character, but also with regard to the important influence which a general knowledge of it, should it be proved true by time, will have on society, I shall proceed to point out the useful results of it under the several heads, as an encouragement to persons to pursue and convince themselves of the truth of statements which the vulgar hastily consider as the chimerical effusions of theoretical fancy, as one of the ephemeral manias of our days. During the inquiry we shall,

shall, I think, remove many objections against the leading doctrines of the system which popular prejudice and misconception have imagined to exist; and shall see that it is a most beautiful and perfect system of anthropology.

Anatomy and Physiology.

Although on a superficial inquiry, many people may doubt of the physiology of the organs or the general results of the system; yet the anatomical discoveries into the structure of the brain and nervous system are matter of absolute demonstration, and will remain a memorial of the industry and perseverance of Dr. Gall and Spurzheim, before whom the anatomy of these organs was wholly unknown. By means of these discoveries we now see the great consistency of Nature in all her productions throughout the creation. Animals endowed with similar faculties have corresponding parts of the brain. Indeed till now comparative anatomy has been a very confused and imperfect science: probably in future a more philosophical arrangement will be made of animals founded on the particular structure of the brain of each class, order, genus, and species.

Of the Plurality and Place of Organs.

The division of the parts of the brain into different organs of the faculties of the mind, and their local arrangement, is strictly philosophical, though the facts on which it is founded were discovered by accident, from time to time. The propensities are all at the lower and back part of the brain and cerebellum. The sentiments at the upper part of the brain, and the knowing and reflecting faculties constituting the intellectual part of our minds, are placed in front. All the organs have been discovered by observing that where a person had this or that part of the head most developed, the same person had particular faculties in a high degree; the particular faculty always corresponding with the organ. It was by such accidents that the local development as connected with particular functions was found out. But though we have this positive proof of the necessity of the development of the parts to the special faculty assigned to them; yet we have not the same proof of local action. Analogy, and the physiognomical expression of the feelings constitute, I think, the greatest proof we can obtain of this fact. To those who dispute the locality of the organs, it may be asked. Have not people in all ages considered the brain as the organ of the mind? Is it not, therefore, more rational to regard it as an assemblage of different organs corresponding to the different manifestations of the mind, than to suppose one simple organ performing such various functions?

If

It seems indeed surprising to me that the brain was not regarded in this light, even before observation had pointed out the particular seats of the organs.

Of the Philosophical Tendency of the Physiology of the Organs.

It may be of use to advert to the metaphysical results of the recent investigations into the physiology of the organs of animal life, with a view to lead to that comprehensive system of the philosophy of the mind, which is the legitimate deduction from premises which an examination of these functions has established. It tends to show,

I. The nature and limits of the influence of external impressions in the production of our ideas; which result, 1st, from the reaction of the organs in consequence of external impressions, adapted by nature to them, constituting perception:—or, 2dly, from the inherent or internal activity of the organs, acting by themselves without external impressions, or from some internal stimulus, constituting variously, as modified by mutual influences, and by the nature of the internal action of the original conceptions of particular genius; which are as it were revelations of sciences and arts from the great size and activity of particular organs. This consideration also explains visions and dreams, which are the consequence of the internal activity. Indeed a very curious history of the varieties of dreams and other internal affections, might be made from considering what organs are active in different cases, and by noticing the effect produced towards waking, when the organs of the reflecting powers begin to act.

II. We see from the physiology of the organs the nature of true and false perceptions. True ideas or perceptions result from the conformity of the action of the organs to the nature of the impressions from without, to which they are adapted. Memory,—a repetition of actions originally excited by external things. Imagination,—new combinations produced by the influence of a distinct and separate faculty on other organs. Genius, or original composition,—the great internal activity of an organ; this is influenced more or less by the organ of ideality, &c. And, lastly, we see that false ideas or impressions must be referred to irregular or disordered action of the organs; which disordered health and misguided education may excite on an organization erroneous in the proportionate development of the different organs. This leads to the consideration of the different mental derangements hereafter to be spoken of.

III. The physiology of the organs shows where certain metaphysical philosophers were right and where wrong in certain opinions; explains the relation between the Berkleian philosophy, which

which denies external matter; and the materialism of Priestley. In other words we hereby see where was the defect of consideration in the disciples of Kant, who were divided about objective and subjective reality. Long before I became acquainted with craniology, these subjects engaged much of my attention, and I felt convinced that a radical difference in the conformation of the mind must be the cause why certain people only regarded the objectivity and others only the subjectivity: and why others saw clearly the reality was the result of the reaction of the impressions of the object on the subject. I know persons who are defective in the organ of Individuality, who, when they are ill of nervous affections, have told me that they felt as if the external world did not exist, but that all ideas were entirely within themselves. All these things however must now be superficially treated of as varieties of insanity, as connected with particular organization and established natural influences.—I mention these facts in a hasty and imperfect manner; they will become the subjects of future consideration of persons more qualified.

Of Insanity.

In Dr. Spurzheim's recent excellent work, he has said, adverting to the treatment of lunatics and the places of their confinement, that they may more peoperly be called mad houses than houses for madmen. The treatment of insane persons is certainly at present very defective and often disgusting to humanity. The discoveries of Gall and Spurzheim seem really to promise some amelioration of their medical treatment. They constitute the only scientific source of knowledge about the varieties of these interesting kinds of diseases.

Of Education.

The application of the physiology of the brain to the education of youth, consists in the cultivation of the intellect, and in the regulation of the moral character; and is founded on the proof we have already obtained from experience, that we can ascertain from the external form of the head the principal and basis of education; namely, the relative development of the different material conditions of the faculties.

I. With regard to the intellect.—Education consists in exercising the faculties. Phrenology, by pointing out the strongest faculties of individuals, will assist us in choosing professions for youth suitable to the genius of the individual; and teach us, 1st, to cultivate those faculties in the exercise whereof he is likely to become eminent; or, 2dly, to give additional excitement to those which, though naturally weak, may be roused into comparative exertion by the excitements offered by education.

II. With

II. With regard to the moral character.—In education we shall be enabled by phrenology to see where, from a preponderance of some particular faculty, there is greater necessity of a counteraction by excitement of antagonist faculties. We learn also how the superior sentiments ought to control the lower propensities, and how the organs of the will should in all cases be exercised early, to give them the greatest range of power over the propensities. We learn also another important branch of education in observing, that to strengthen and render habitual any good feeling, as benevolence for instance, we must not only inculcate it but must expose the child to objects of charity, and enhance it by setting before him instances of mercy. Thus phrenology comprises the sentiment the wise have held in all ages, of the poverty of precept when compared with example, as an incentive to moral excellence.

Of Punishment.

Phrenology will lead to important considerations regarding criminal punishment; particularly in houses of correction. It will enable us to distinguish, not only between those who have naturally strong evil propensities, from those whom distress or other contingences may have hurried on to crime; but will point out the particular nature of many evil propensities to be corrected. It is hoped that the learned authors of this system will more fully develop, in some future publication, its particular application to punition.

Conclusion.

Having shown some of the particular useful purposes of the study of the anatomy and physiology of the organs of the mind, I may observe, in conclusion, that while, from the most accurate and continued examination of the subject, I feel fully convinced that the conclusions are legitimate deductions from demonstrable facts, and ought for that reason alone to be studied and applied, whatever might be their supposed tendency; I am nevertheless convinced that the objections raised against the deductions, by persons who fancy they will be injurious to the religion and morals of mankind, are wholly unfounded, and are the result of a very superficial examination of the subject; and that, on the contrary, it is the wild metaphysical dogmas of the modern schools against which the shafts of this accurate scrutiny into the nature and functions of mind are most successfully levelled. That, in short, the moral results of the system constitute a scientific explanation, and therefore confirmation of doctrines which common sense has always inclined to, which religions have inculcated, and which the popular and proverbial philosophy of the common people have

 expressed

expressed on many occasions more clearly than the perplexed jargon of the schools.

St. Bartholomew's Hospital,
27th Jan. 1815.

T. FORSTER.

P. S.—I have omitted entering into the particular proofs of the doctrine, as this would be trespassing on Dr. Spurzheim's lectures, which every candid inquirer should attend himself before he makes up his mind on this subject. Even if the doctrine were false, these lectures contain sufficient interesting matter to repay the trouble of attending them. I have only to apologize for these very hasty and imperfect observations, which the time allotted for a periodical publication will not allow me to revise, and to which I should not affix my name, had not several persons carelessly ascribed many unsigned letters on this subject from artists and medical and other persons, on its particular bearings, to its authors themselves, and have thus accused them of an anonymous encomium of their own discoveries. I publish them only to invite students to examine the facts for themselves, and verify their truth or falsehood by their own investigations.

XIII. Dr. SPURZHEIM's *demonstrative Course of Lectures on Drs. GALL and SPURZHEIM's Physiognomonical System.*

[Continued from vol. xliv. p. 370.]

Lecture 10. HAVING finished the knowing or perceiving faculties which are common to all animals, Dr. S. proceeded to the reflecting faculties; philosophers call the latter reason, and the former understanding. Animals have understanding and some reason. Examples of dogs; one mentioned in the French translation of Locke's Essays, where a dog that was secluded by others from the fire, went out and barked so as to attract the attention of all the rest, and immediately returned and then took possession of the best place near the fire. Other instances of reasoning in animals were also mentioned, furnishing unequivocal proofs that animals reason, but in an inferior degree. Man is more than an animal. Men use facts; one states a fact, and is satisfied; another adduces a similarity or analogy, and convinces all who hear him. Men of fact have the lower part of the forehead prominent over the eyebrows; men of analogy and parable have a kind of inverted pyramid at the top front of the forehead; popular preachers have this part developed, parables and similes being the best means of conveying knowledge to the vulgar, and was adopted by Christ.

This

This faculty compares, it is the XXXth configuration or *organ of comparison.* Animals have it in a certain degree; it is necessary for reasoning to compare, and men love comparisons.

If the upper part of the forehead be prominent, or an elevated ridge traverse it horizontally, it is what Gall called the organ of metaphysics; but all men are more or less metaphysicians. The lateral prominences Dr. S. considers as indicating the relation between cause and effect; while the centre elevation compares, the side ones reflect on causes and effects. Metaphysicians go too far, they pretend to know God whom it is impossible for man to know; they take virtues only for causes. It is injurious to the progress of science to inquire into causes instead of observing facts. This is the XXXIst *organ of causality;* it appears in every artist, and mechanic, &c. all excellence in any art or calling is indicated by this organ; Dryden, Milton, Shakespeare, Locke, late Mr. Pitt, &c. had it. Philosophical minds have individuality, facts and analogy or relation between cause and effect; *i. e.* knowing, comparing and examining the cause; these are confined to the centre; it is not necessary to have the whole forehead prominent; the mere front is sufficient. Instance of a shepherd with highly developed reflecting faculties.

XXXII. *Organ of wit;* it is situated in the lateral parts, which must be prominent over the eyes, as appears on Sterne's temples. What is wit? it is very difficult to define; many writers have vainly attempted to give the definition in words; wit and humour are only different degrees of the same faculty. Wits have a peculiar mode of comparing; wit is often false, yet it stimulates to laughter.

XXXIII. *Organ of imitation;* it was discovered by Gall on examining the head of a player who had a deep furrow on the top of his forehead. Instance of a deaf boy, who imitated or mimicked the manner of all the patients brought to the hospital in Vienna where he was. Bust of Shakespeare having this organ, that of benevolence and adjoining parts high; it is useful to artists, sculptors, &c. it gives them the power of producing expression. Sculptors ought also to have the organs of construction and imitation. Children do much by imitation, and the majority of men live all their lives by imitating others, instead of reflecting for themselves.—Having finished the definition and demonstration of the organs, Dr. S. observed that of some he had spoken positively, of others only as probable, and of some again as very likely. The entire system is founded on facts and experience; prove the facts false and it is annihilated. Dr. S. has added three organs to the number proposed by Dr. Gall, and made them thirty-three. Philosophers proceed one way, naturalists another; the latter inquire, on seeing any new

 subject,

subject, To what order, genus, species or variety does it belong? He, in like manner, begins by inquiring how many organs are indicated by the mind? It is necessary to multiply organs till all the primitive faculties are explained. The applications of these faculties are indefinite. There are thirty-three organs; and as the letters of the alphabet are sufficient to indicate sounds and form words, so are they to signify the faculties. The propensities influence the character; sentiments also influence the reasoning faculties. Man is a determinate being; education is not capable of making the character, and teachers all know this. Animals likewise have a determinate nature; the faculties are innate; "he who has an eye can see; it is given to him," according to the Scriptures. God has done for man what he did for other animals, given him instinct, however he may be too proud to allow it. Education, indeed, is nevertheless necessary to put the faculties in action. That genius is born, is a trite truth; education never creates, it only cultivates and directs the faculties. All faculties and moral feelings depend on organization. The organ of language is a transverse convolution of the brain very distinct from the anterior lobes. Every faculty has a peculiar organization. No feeling is learned by the understanding. Understanding and order may exist without good feelings; others without great genius have good feelings; hence the reason why men appear weak in some particular and not in others. Some persons reason better and feel more when sleeping than when awake; this fact is explained by the circumstance of some faculty acting with more energy when alone than when all the others are engaged. For instance, we retire to quietude to reflect; we adopt some means or other to aid us in every undertaking. Hence, animal magnetism. We do not yet know the vital power, nor the extent of the influence of mind on matter. All nature is regular. Animal magnetizers are ignorant of nature; they pretend to great knowledge, but have never discovered any thing; they know nothing new. But every one has his gift.

Lect. 11. The organs of all the faculties are situated together in subservience to their mutual influence; thus, the reflecting faculties are together; the knowing ones together; and the propensities, &c. in like manner. The organs of knowing and reflecting are in the fore part of the head; those of the propensities and sentiments in the upper and posterior part. The faculties peculiar to man occupy a very small space; those common to man and animals much larger, and some faculties are more important than others. It is very difficult to find proper terms; those of affection, passion, memory, &c. are not

facts,

facts, they are only names of modes of the primitive faculties. In like manner quietness, temperance, &c. only mean the different degrees of activity in the faculty. Passion is the highest degree of activity of any faculty; a man devoted to music, painting, &c. is passionately fond of those arts; and so of all others. Pleasure or the state of being pleased does not indicate the faculty or the activity of the faculty; it is therefore only a mode of activity. A religious man is pleased with religion, &c. a faculty unoccupied is displeased; hence men are displeased with every thing which does not engage their faculties, or, in the familiar phrase, for which they have no taste. No faculty peculiar to man can be angry; anger is only a mode of action, an affection of combativeness; anxiety, sorrow, &c. belong to cautiousness, and compassion to benevolence; jealousy arises from covetiveness and self-love, from amativeness if relating to women, or from approbation if disappointed in praise; envy is jealousy uninfluenced by benevolence; shame is a compound affection arising from self-love and approbation. Attention is different in different animals; every creature has its peculiar attention to some things. If faculty is active, then there is attention which only indicates activity. We cannot command attention to any thing at all times; there is no faculty of attention, but an attention to every faculty. Perception is the knowledge of an object carried to the brain, reflection reproducing this perception is memory; one remembers names, another forms, a third numbers, &c. there being different kinds of memory according to the particular individual. Men who reflect remember better than those who do not; this is occasioned by the mutual influence of the faculties. Perception is the first degree of activity, memory or remembrance the second; difference between memory and remembrance, we remember a fact without having memory sufficient to repeat all the circumstances; many persons remember having learned a song, but cannot repeat it; others know the name of any person or thing, yet cannot mention it, but they recollect some circumstance which proves their knowledge of the fact; remembrance belongs to the faculty of individuality; memory to the perception only. Imagination belongs to the third or highest degree of activity; there are only three degrees of activity, although Gall made four in each organ. Internal propensities in man are instinct in brutes. Judgement is found in the highest and lowest degree of activity; a good judgement often exists without a good memory, and *vice versâ*; hence it is a mode of activity. The relation between the knowing faculties and external objects is judgement; every intellectual faculty has a judgement; but because the reflecting faculties are more active, we call him who judges correctly, a person of good judgement.

Hatred is a disagreeable affection of all the faculties; revenge is the activity of the vindictive passions, of combativeness without benevolence, the presence of all the bad and absence of all the good propensities.

Lect. 12. Those who wish to study this system, to know the development of the organs, must consider every dimension; look at the forehead if high or broad, then in profile, and draw lines from the ear to the eyes, top of the forehead and crown and occiput; next divide the head into three divisions, which are called the upper, middle, and lower regions.—Diversity in the development of the feelings common to men and to animals according as there are diverse characters among men. Some of the five senses are more indispensable than others; men can bear being told that they are destitute of one talent or another, but none can patiently bear to be called stupid. There are three kinds of faculties with respect to their actions, or rather three kinds of action; one assists men and animals, another is auxiliary to those faculties, and the third or superior directs the whole. Throughout nature all the superior laws govern the inferior; hence the animal part of man, *i.e.* all the faculties common to men and brutes, should be governed by those faculties peculiar to man only.—Examination of human liberty and moral good and evil. "All faculties are given;" this is the language of Scripture: but, Does this truth lead to fatalism? St. Augustin and the Christian fathers all contend for it, and deny that it has any tendency to diminish man's responsibility for his actions, or to make him a machine: "Every one according to his talent;" all things are given, the feelings and intellects are given at creation; if this be called fatalism, the fact is admitted; but it is not an irresistible fate; neither feelings nor intellects are obliged to act; muscular power is not compelled to act, it is not necessarily obliged to produce muscular motion; hence we find it admits of liberty, but what kind of liberty? not liberty without motives; there is no such thing as absolute liberty, but a liberty to act as feeling directs, a liberty *by* motives, and not *without* motives; which would be irrational. Every faculty is pleased with action although not absolutely free; hence the precepts of religion are applied to the motive; because faculties are given they are not therefore impelled to act. Pleasure is not freedom: when a dog declines to follow a hare, he then acts free; in proportion as the understanding is active men are free, the more understanding the more freedom. Yet, having understanding is not freedom, it is only having greater power of choosing among motives. Men who have the greatest number of motives are the freest; but, Whence are the motives? From the propensities

and

and the feelings. Animals are free, but man has moral liberty. Free from the inferior feelings you are the slave of the superior ones, as observed by St. Paul. No religion makes man answerable for his inclinations but for his actions; feelings are not excited by the will—we may desire to be very pious, very benevolent, &c. but cannot create such feelings in ourselves by means of our will. The instruments on which the feelings act render man responsible; hence, as liberty or freedom commences, so does moral good or evil. Physical and moral evil exist, but whence the origin of evil? No tenable theory of this problem has yet been produced; it is not from two creators, it is not from freedom,—this cannot be the cause, as there is nothing in nature bad; evil is not a substance, it is not the name of any thing, but the bad use of a faculty. Neither is there any bad faculty, yet evil exists. Animals do the same things as men, yet they are not called evil. Sin exists in man as the animal predominates over the human. Man is inclined to evil as the animal organs are much greater than the human ones; hence evil exists, has existed, and will exist. It is the human or higher faculties only, which give laws; the virtues are commanded, the vices prohibited; "thou shalt love the Lord;" "thou shalt not kill;" one commands the higher faculties, the other prohibits the inferior ones; hence, we sometimes do by the impulse of the inferior feelings what we do not like, and what is condemned by the superior ones. Man sometimes does without law because the animal is small in him, his mind is his law; hence we see that virtue is possible; but it is now impossible to abolish law, and therefore it is not to be abolished but fulfilled, till men be born again, till the reign of love arrives, as they are now in general more governed by the animal than the human faculties. Who is entitled to give laws? he who has the superior feelings in the highest degree. What is the aim of our faculties? many of them are given for our preservation and that of society; neither is our aim all selfishness, as many modern philosophers, particularly Helvetius, pretend. Others attribute all to Christian charity: this is no less untenable; some again reduce all our actions to faith. These are merely individual and not general opinions. No actions are good by faith; there are several motives for our actions; men in general act from their feelings, very few by their will or understanding. All our judgements of men are the result of moral feelings and not of the will; it is the feeling which disposes men to religion, as the greatest intellect is not necessarily the most devout. Hence it may be determined whether this doctrine of physiognomy be dangerous to morality and religion in general, or whether it does not furnish new and convincing proofs of both being natural to man. We also see that it elucidates

the meaning of many parts of the Christian revelation, the sense of which was nearly lost or very inadequately comprehended.

Lect. 13. With respect to the mutual influence of the faculties, the difference between the organs and the conditions under which they act must be considered. There are four considerations relative to mutual influence; first, every faculty in every individual is modified, not only in different individuals but also in the same individual; in like manner different parts of different animals are modified in the five senses, and also in the faculties of the mind; habitivenes, constructiveness, destructiveness, &c. are modified in all animals; some birds construct their nests according to circumstances; others have their singing powers modified and different from those of the same species; some tastes are peculiar; various idiosyncrasies, &c. The same diversity takes place in the mind. Some comprehend or understand matters when the facts are duly arranged; in others the faculties themselves are modified. Two mothers having equal affection for their children, but one having more destructiveness and less benevolence than the other, will treat them very differently. Maternal love never acts alone; if a mother shows a partiality for one child, it is not that she has no affection for the others, but because this favourite is the most congenial, the most similar to her own disposition. Here size is not sufficient to determine the actions; destructiveness however prominent is modified by benevolence, justice, veneration, &c. in various degrees. Self-love is not in itself bad; in the man who esteems justice, feels benevolence and veneration, it is very good as it respects only those qualities either in self or others; but self-love without benevolence, the forehead low and little faith, veneration, or justice, is very bad. We may thus discover whether justice be predominant or feeble in legislators. Veneration alone without energy produces credulity and superstition, one who will worship any thing that is offered to him. When veneration is prominent and the posterior parts of the head projecting, it produces more than piety, it makes an inquisitor, (the Professor exhibited a real portrait of one,) who will murder his fellow men for the glory of God! A true Christian minister has the forehead very large, posterior very small. The intellectual faculties are also modified; thus, among musicians one has a genius for poetical or ideal, martial, philosophical, or tender music. An author or writer with covetiveness is a plagiarist; without benevolence forms a satirist, with benevolence it is not satirical. The faculty of language never acts alone; it belongs first to nations and then to individuals of a nation. One nation has more feeling and peculiar ideas than another. Individuality is the first developed faculty; nouns and verbs are the first words used by children,

children, and Horne Tooke reduces all words to these two classes. The French language has no analogy, no comparison, no classification; Frenchmen's organ of individuality is most developed; they always begin a discourse with the fact, and never with the *cause;* they say this or that happened without ever saying *because* it was so or so; there is no connection or chain of analogy and causation in their discourses or minds. All the faculties act in conjunction and not alone. This appears even in their abuses. He who has covetiveness with veneration never steals in a church, never commits sacrilege; no, he observes, "it is not good to steal in a church;" another who has covetiveness with benevolence says, "it does harm to no one," and steals there. We may also distinguish the chief of a banditti by his pride and firmness, which he must possess in a high degree. But no faculty can be judged alone. It is extremely difficult to judge; all men hitherto judge others according to their own associations; in fact they judge themselves in others. Even God is made, according to their own feelings, a warrior or a peace-maker. This appears even in the Scriptures; St. Peter's God is fear, St. John's love. But we must henceforward judge men by the predominance or remissness of the superior faculties, whether the faculties peculiar to the animal or those peculiar to the man have the ascendency. Hence we have a measure, a standard of judging. Faculties may be excited by circumstances, and some are taught music as a trade, and not by innate genius: yet Handel would have composed had none existed before him, and Zerah Colburn calculates by some process not yet taught by men. The world or sphere of action is in relation to the quantity of active faculties; he who has most active faculties has the greatest world. Indulgence must be extended to all persons for their contrariety of opinions; they cannot help it. The doctrine of association of ideas has occasioned many erroneous conclusions. As evil is not a substance but an action, so likewise an association takes place, but it is not a thing; one propensity excites another, one sentiment another, and one intellectual faculty another; seeing an object excites the faculty of language; places excite the organ of space, &c. Hence the basis of all mnemonics. Colour is always present to Frenchmen.—Characters:—a modest man has benevolence, cautiousness, and little self-love; a candid one, all the superior feelings, and a roundish outline; a serious, grave with firmness; a "touchy character" has much self-love and approbation; one who never bows, self-love, destructiveness, firmness; one who never pardons, vindictiveness, distrustfulness, &c. In all languages there are more names for bad than good characters in men.—Sympathy and antipathy; every faculty desires to be satisfied, and so far as it is satisfied we like or dislike others.

Some

Some faculties are social, some antisocial; the former are human, the latter brutal. The selfish will not go with the selfish, nor the proud with the proud; no proud man likes pride, and hence the origin of antipathy. The credulous, modest, or circumspect, never love the selfish, extravagant, &c. Similarity or congeniality is the basis of friendship. The more faculties a man has the more happy he is; but if these faculties cannot be gratified, then he is in the same proportion miserable.

Lect. 14. The Professor now proceeded to unfold the second part of his system, the external expression of the activity of the faculties. This has been called physiognomy, pathognomy, mimickry, &c. Dr. S. denominates it pathognomy or natural language. Whenever any faculty is active it manifests itself by external signs, called pathognomical. Lavater admitted to the number of 30 signs, every one of which formed a kind of exception to the others; but the laws of nature are certain and have no exceptions. Physiognomy is a very ancient study; Solomon's Proverbs contain examples of it. Pathognomy or natural language manifests the internal activity of the faculties. Here the Professor took a retrospect of the principles he had advanced, described the connection between the spinal marrow and the brain, and that the latter acts on the body by means of the nerves. The five senses are influenced by or act on the external world. Whenever any propensity is active the necessary means also enter into action, the five senses and voluntary motion; hence mimickry or pathognomy gives the proper attitude. Nature always adopts the best and easiest mode, without any reflection; it is perfectly instinctive.—Examples of false attitudes given to their statues by the ancients. Throughout all nature the faculty is conformable to the action and the action to the faculty, and the motion or gesture is always in direction with the peculiar organ or faculty in action, whether it be seated upwards, downwards, or on the side. Attachment or friendship manifests itself by inclining the body and shaking hands, &c.; combativeness instinctively puts itself in an attitude of defence, contracting the forward muscles, drawing in the arms, &c.; destructiveness when in action both in men and animals, opens the hands and claws like grasping, shakes the head, &c.; a man having secretiveness wishes to conceal himself, knocks gently at the door, walks on tiptoe, speaks low, and when departing contracts himself; cautiousness looks and listens on all sides, pauses, looks, and listens again; in approbation all is slight, delicate, attentive; but self-love is stiff, reserved, careless, unbending, and the reverse of approbation; the reflecting faculties withdraw from the external world, shut the eyes, become still, &c. When different faculties are in action, all similar parts are also active.

It

It is very difficult to make two different motions at the same time. No one can restrain the external manifestations of his internal feelings; a general concordance with the feeling likewise takes place in the voice, hands, &c. and voluntary motion.—Examples of deceit: when a person assures another of his friendship with a voice and tone not in concord, the voice indicates the feelings; approbation is insinuating, self-love indifferent.—The different manifestations are also modified by the different situation of the organs; adhesiveness or attachment inclines the head and to one side, as a mother to her child; self-love holds the head up and backwards, in reading is erect, with difficulty bows the head, and then it is only a nod; the humble man bows lowly, your very humble servant; approbation or vanity turns and smiles on both sides looking for attention; firmness stands up steady; veneration turns the eyes and hands upwards and forwards, this cannot be explained by the tradition of God being above us, for he is every where; neither by our position, for every twelve hours we are antipodes to ourselves, but by the sympathetic influence of the act and the organ at the top of the head; it is the faculty which gives the internal impulse. In the reflecting faculties whenever we are puzzled to recollect a name or circumstance, we immediately rub our eyes, within which is the organ of memory, and never the top of the head; when a man cannot comprehend a thing he strikes his forehead exclaiming, "how stupid I am!" Travellers put their finger on the organ of space. When an artist looks at a façade he first considers the order and symmetry, then the construction, and immediately turns his head from side to side, as this organ is on both sides. Sterne is represented with his finger on the organ of wit. The essential character is similar in all countries, and is only modified by nationality. Characters however are not to be pronounced from the action of one faculty at one moment. Mimickry or pathognomy may be permanent or transitory. Pathognomy is natural language, pantomime is artificial; thus washing the hands to indicate innocence or freedom from guilt is pantomimic, it is a thing previously agreed on and is artificial; whereas pathognomy is natural and involuntary. Artists attend to grace and neglect truth. The study of physiognomy is difficult, as there are many signs and those compound to be understood; they are less numerous than the terms in botany and the species of plants. Neither is it so difficult when reduced to rule as now attempted. Physiognomy is accused of cruelty to individuals of adverse configurations; but natural truth can never be injurious or unjust. He who knows man best is more indulgent than he who knows him least. The deaf and dumb have the same affections as others, and understand their characters as well. Express anger or pride, and the deaf or dumb will instantly feel and understand

understand it. Actors and artists should know this, to make the imitation of what is desired.—Expressions of agreeable and disagreeable; we inspire an agreeable smell, we expire a disagreeable one, and so with all other senses and faculties. All disagreeable things contract the body. *Yes* or *No*. The former inclines the body forward, the latter reclines back and shakes it off.

Lect 15. The important subject of education now engaged the Professor's attention. The nature of man is determinate.—He may reduce all knowledge to two ideas; external circumstances and education. Savage or wild men are said to be ignorant because they have received no education, but it is because they are deficient in natural faculties. The wild boy of Avignon is still ignorant and silly. Idiots sometimes wander into the woods and become like savages feeding on vegetables, &c. It is want of talent and not education that occasions this state. But, education is extremely important, although it is given. Are we all of the same species? Caribs and Negroes are different, yet in them all the organs of the brain are found, and therefore they cannot be a different species. In future, the nervous system must be the basis of all classification; the nerves we find similar, and accordingly some Negroes are mathematicians, &c. and consequently not of a different species.—All men in general similar; then how shall we perfect man? Nature is the same now as in ancient times; we can neither take away nor give a faculty; we can only cultivate one and suppress another, and by this means not only individuals but whole nations may be so far perfected.—It must be admitted that very little success has hitherto attended our efforts in education, notwithstanding what has been written on the subject. The knowledge of man is still very backward, and consequently so is education; the cause is obvious; we have pursued an erroneous method. We must begin where we now end. How do we improve the species of animals? by innateness. It has long been deemed desirable to belong to certain families where there are no hereditary diseases. If there be hereditary diseases then there must be hereditary organization, and therefore innateness. In improving plants we consider the seed; sow it in the same soil and nature furnishes genuses, one plant is very fine, another is indifferent; we preserve the seed of the good and reject that of the bad. Ingrafted trees are used to improve the quality of the fruit; yet they ultimately degenerate and die, as the stock is unsound on which the graft was placed. To improve wool, we begin by improving the breed and blood of the sheep, and not by changing their food; here we begin with birth and race; we should do the same with man. The ancients were attentive to this in their marriages. Moses speaks of sons of God and daughters of men; Aristotle and Plato observe it.—

There

There is, indeed, great difficulty in establishing other modes of education, owing to external circumstances. Nevertheless we must expect the same effects, while the same causes exist. Education is commonly but not correctly divided into physical and moral: we divide it into, first, whatever contributes to greater energy; and, secondly, whatever contributes to the direction of the faculties. The first is effected by innateness, by family or lineage; the second is physical education, commonly so called. All that contributes to improve the body, perfects the developments of the mind (not mind itself); all external circumstances regulated according to the real nature of man. Food is very important, as in feeding cows we perceive a great difference in their milk; the flesh of sheep fed on mountainous districts is better than that fed in valleys; now, Why may not similar effects of food take place on the nervous system? This is properly what is called temperament, which gives energy. Climate may also have its effects. We find the fruits of warm climates are more savoury than those of England, where they are cultivated in hothouses which they cannot bear in warmer climates. One climate may suit the development of one faculty better than another; certain meats may have the same effects on the nerves, and there is no reason why food should not be classed according to its effects, or be found to produce certain effects, the same as drugs are classed according to their effects on the different parts of the body in the diseased state, as stomachics, diuretics, emmenagogues, &c. There is, however, some difficulty in fixing this point; our knowledge is defective; yet external circumstances are essential, as contributing mediately to improve the energy of the mind.

The third condition is exercise; what is it? putting in action. The propensities and sentiments are never taught in schools, but only exercise of the intellectual faculties. Yet if you continually speak of music to a student, and at the same time withhold all instruments from him, you will never make him a musician. Speak to a boy of hunger, but till you withhold from him food, he will never understand you; give him little food, and he will feel it. Benevolence is more common among the poor than the rich, because the latter have this feeling less excited; they have less opportunity of knowing and feeling misery. Bad company exercises the faculties and inferior propensities. Veneration is not taught in a gambling-house. Example is ever more important than all the precepts of education. You cannot exercise all the faculties at the same time. Some faculties are late, others early developed; some last during life, others diminish with age. When some men grow old, they grow more virtuous as they suppose; but it is only when their sins have left them; vir-

tue

tue consists in governing the propensities or faculties when they are most active, and not when they become dormant. Children soon develop benevolence, approbation, cautiousness, individuality, combativeness, and even destructiveness. Veneration is generally later of being developed; but some one faculty occasionally appears at an early period. There are other conditions, such as the mutual influence of the faculties, as a mean of inducing their greater activity; the faculties may act from external impulse, but much oftener from the mutual influence of the parts. Does exercise increase the dimensions? It is possible and probable, as we know it improves agility. We occasionally see heads which we regret had not been exercised in youth. The basis of all moral actions is submitting the animal to the human faculties; man governs, the animal executes. Men never act without motives, which are generally very different. If actions cannot be produced by love, and the higher faculties, then the animal ones must be called in to their aid. Will consists of the faculties of individuality, perceiving and comparing; hence a reflecting man has more will, as he anticipates things, and is therefore not the slave of motives. His feelings furnish the motives, but his will has the direction of them, although not of his feelings. If in a youth benevolence be developed, use this faculty and not veneration, as exercise cannot take place before the organs be developed; if approbation, you may exercise and modulate, but cannot extirpate it. Teachers and parents often put children in circumstances to *exercise* approbation at the very moment when they are speaking against it. Direct, but seek not to destroy the inferior faculties. If mature persons are to be taught, use their most developed sentiments; if benevolence use it, if veneration, religion, &c. to enable them always to direct and govern the animal faculties, keeping them subordinate to those of the man. There are a few individuals who may be called elect; naturally it is very difficult for them to do much evil, as they have all the human faculties, and very few of the animal ones; such persons are, as St. Paul says, the slaves of righteousness, they are a law to themselves, and are elect. Children often have great combats between the animal and the man; and even where the animal prevails, yet noble actions sometimes appear. Some children can never be prevented from crimes; but the great majority adopt the habits of their fathers, and follow their own customs. Take care of those in whom the animal is ascendant, lest they enter into similar company, where those propensities will be exercised and augmented instead of being oppressed. Not every one is fit for every thing; this fact is much neglected. Some persons are forced into the learned professions when they prefer the mechanic arts. But all the

faculties

faculties should be exercised even for the lowest mechanic, as he who reflects will always be a better tradesman than he who acts by imitation and habit; all men should be taught to think. Professions should be studied; peculiar judgement depends on peculiar organs, but a philosophical judgement does not. Education on these principles would diminish crimes, but not make men angels; for this they must be born again. In the actual state of man it is impossible to prevent errors entirely. Here the Professor exhibited six skulls of Germans, six of Frenchmen, and six casts of Egyptian mummies, Caribs and Calmucs, all of which were dissimilar; hence the impossibility of determining national character from such skulls; nor has Dr. S. ever seen six skulls of one nation in any of the cabinets of those naturalists who pretend that the native country of any subject may be discovered by the figure of the skull. In general, however, there is a national configuration.

[To be continued.]

XIV. *Notices respecting New Books.*

Mr. Thomas Forster, a most indefatigable and successful cultivator of the natural sciences, has just given another proof of his zeal to extend natural knowledge and benefit Society, by publishing a neat Edition of a Greek Poem, The Diosemeia of Aratus; illustrated with numerous Notes, collected from various Authors, ancient and modern, respecting meteorology, atmospheric phænomena, and their corresponding effects on several animals, birds, &c. Those who have read this ingenious writer's elaborate "Observations on the Brumal Retreat of the Swallow" may form some conception of the multifarious notes with which he has illustrated the Greek text of Aratus.

The same Gentleman has in the press, and will publish in a few days, An enlarged Edition of his Researches about Atmospheric Phænomena. Having noticed in the pictures of even some of the most celebrated landscape painters, a great error frequently in the arrangements of the clouds, he will subjoin in the new Edition, some rough copper-plate engravings of the different modifications of clouds, illustrative of the electrical theory of their formation and changes.

In the press, and will be published early in February, "Dissertations and Letters, by Don Joseph Rodriguez, the Chevalier Delambre, Dr. Thomas Thomson, Dr. Olinthus Gregory, and others;

others; tending either to impugn or to defend the Trigonometrical Survey of England and Wales, carrying on by Colonel Mudge and Captain Colby; collected, with Notes and Observations, including an Exposure of the Misrepresentations and Contradictions of Dr. Thomson," by Dr. Gregory, of the Royal Military Academy.

Mr. Robertson Buchanan, Civil Engineer, Glasgow, is preparing for publication "A Treatise on the Building, or Hydraulic Architecture, of Water-Wheels, containing detailed Descriptions of their component Parts, and Descriptions of a great variety of Water-Wheels, with practical Observations, and, in some cases, particular Specifications for the Direction of Workmen, and the Calculation of Estimates. Illustrated with Plates."

This Work is intended more particularly for the Use of Millwrights, and Students of mechanical Drawing.

To which will be added, A Treatise on Machinery for propelling Vessels, more especially as it relates to those termed Steam Boats. Fully illustrated by Plates.

The same Gentleman is also preparing for publication "An Essay on the Œconomy of Fuel, more especially as it relates to Heating and Drying by means of Steam." In three Parts.

1. On the Effects of Heat, the Means of Measuring it, the comparative Quantity of Heat produced by different Kinds of Fuel, Gas Light, &c.

2. On Heating Mills, Dwelling-houses, and public Buildings.

3. On Drying and Heating by Steam.

Illustrated with two Plates. With Additions, containing the most recent Discoveries and Improvements.

The following interesting works are nearly ready for publication:

"Travels in Europe and Africa, by Col. Keatinge." This work will be illustrated with numerous Engravings of Antiquities, Scenery and Costume, from Drawings taken on the Spot.

"A Supplement to the Memoirs of the Life, Writings, Discourses, and Professional Works of Sir Joshua Reynolds." By James Northcote, Esq. 4to.

"Private Education, or The Studies of young Ladies considered." By Elizabeth Appleton, late Governess in the Family of the Earl of Leven and Melville.

The second and concluding volume of Professor Lichtenstein's Travels in South Africa.

XV. *Pro-*

XV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Jan. 12. The President in the chair.—The conclusion was read of Mr. Travers's elaborate and ingenious paper on the mechanism of the eye, and the means by which this organ obtains distinct vision of objects at different distances. In concluding, he took a summary view of the principal hypotheses formed to explain this phænomenon, such as the muscles of the eye, the ciliary processes, and the supposed muscles of the lens itself, by which it was capable of receiving distinct images of objects at different distances. All these opinions Mr. T. showed were totally incompatible with the facts demonstrated by anatomy; he also proved that the supposed influence of the will over the muscles of the eye, impelling them to contract or dilate at pleasure, can have no better foundation, as the mechanical action of light on the eye produced this effect without any regard and often even in direct opposition to the will, and is attended with a painful sensation in the eye: besides, this supposition assigns two causes for one effect, which is unphilosophical as well as contrary to physical experience. Mr. T. then described the anatomical structure of the eye, observed the muscular appearance of the iris (Berzelius in his Animal Chemistry considered this organ as muscular), inferred that there is a sphincter muscle, and that this muscle produces an external pressure on the lens, which alters its shape, and thus effects distinct vision by adjusting the pupil to different foci. The contraction or dilatation he considered as being always in proportion to the quantity of light reflected from external objects, and not according to the will of the observer. The crystalline lens, being more fluid in youth than in old age, sufficiently accounts for the diversity of vision in those stages of human life. As the nearer objects are to the eye, the more they reflect light on it, and hence the painful sensation is occasioned by continued looking in this situation.

Jan. 19. A letter from Dr. Brewster to the President was read, containing an account of some more experiments, by this indefatigable philosopher, on the effects of compressing animal substances which depolarize light. The principal experiment was on calf's foot jelly cut into a columnar form and compressed between two plates of glass: at first no depolarization of light took place; but after the jelly had remained ten days between the glasses, and acquired a consistency almost approaching that of caoutchouc, the edges evinced the commencement of depolarization; and when the whole mass had attained a uniform den-

sity, the depolarization was complete, and the jelly had acquired the property of depolarizing light in every direction, like other objects before operated on by the author.

A letter from Dr. Storer was read, describing the nature and circumstances of a spring of fresh water discovered in the harbour of Bridlington, Yorkshire, a few years ago, when boring in order to ascertain the practicability of making some improvements in the harbour. The tide here flows to the height of fourteen feet, and the boring was commenced near the low-water mark; the soil is a stiff blue clay, and when the auger had passed through about twenty-eight feet, it struck on a very hard rock, and the operation was discontinued. At first there was no appearance of water; but after a few hours fine fresh water began to rise, and rose so rapidly that it flowed over the side of the works raised for the experiment. As Bridlington was not well supplied with water, a tube was inserted in the bore, the parts around it covered over, and the conveyance made for the water to run into reservoirs when the tide flowed. This spring has now continued to flow with the tide some years, and supply the town with good fresh water; when the tide ebbs it ceases to flow, and as the tide rises so does the water in the spring. In summer many of the wells in the neighbourhood are dry, but this remains permanent. When great floods occur in consequence of the rains at the end of autumn, the water of this well rises higher; which is the only change that it experiences. The explanation of this appearance offered by Mr. Milne, the architect of the works, is, that the whole bay extending to a considerable distance is covered with the same stratum of blue clay which ultimately terminates in the sea on the rock which runs under the spring, and that the rise or flow of the water in the well is occasioned by the superior gravity and pressure of the tide, acting like as the two arms of a syphon charged with two fluids of different specific gravities. Dr. Storer admits this explanation as generally satisfactory; but observes that, according to this theory, the well instead of flowing higher during floods should not rise so high, as there must be a greater quantity of fresh to resist the action of the salt water.

The Doctor's objection seems founded on the notion that the increased quantity of fresh water should also increase its specific gravity; but it is the volume only which is increased, and hence an increased flow from the well.

Jan. 26. Part of a paper by Mr. Cooke was read, proposing some methods of improving marine charts, and facilitating their general use. It also contained some suggestions for taking angles from light-houses with the pole star, and coasting at night, &c. many of which necessity had long ago anticipated. He

He proposes that charts should be varnished, but did not state the kind of varnish that would bear rolling and unrolling repeatedly.

Signor Zamboni presented to the Society an instrument of his own construction, being an attempt to exhibit a perpetual motion. The principle on which it acts has been known in this country several years. It merely consists of two of De Luc's electrical columns or galvanic piles, placed perpendicularly at the distance of about six inches, and each glass tube is surmounted with a brass ball; between these pillars a steel needle is placed to move on an axis; the longer arm of this needle touches the upper end or ball of each pile, and receives from it a sufficient repellent force to drive it to the adjacent ball, and *vice versâ*: in this manner the motion is continued. No apparatus to measure time has yet been connected with this simple motion, which is protected from the atmosphere by a glass frame.

XVI. *Intelligence and Miscellaneous Articles.*

EXTRACT OF A LETTER FROM M. VAN MONS TO MR. TILLOCH*.

SIR,—" YOU are probably acquainted with the new discoveries made at Milan, by Messrs. Moscati and Maury, relative to the sun, its diurnal or rotatory motion, its volcanos, &c. I send you a translation of the report published on this head; I also send you a note to what I have said on the metallo-fluores, and another on my new theory, which is that of caloric considered as a constituent part of all bodies containing oxygen, displaced in the combinations and displacing itself in the decombinations. Afterwards I admit hydrogen reduced into its gas into all the combustible bodies, and into the metals, and sub-saturated in all bodies which can, in their quality of bases, contract combinations. Hydrogen gas is a simple body; oxygen gas is composed of equal parts of oxygen and of caloric: the primitive material of the globe, and without doubt the substance of the other planets, also consists of equal parts of oxygen and hydrogen, without the least quantity of caloric, which would break this relation: water is oxygen gas displaced in the ratio of $\frac{2}{15}$ from its caloric by two of hydrogen, and there result in this way 15 parts of oxygen, 13 of caloric, and 2 of hydrogen. The

* Much valuable scientific information from various parts of Europe will be found in this communication; and we are happy to be able to state, that M. Van Mons has promised to continue his correspondence. The further materials alluded to in the present letter have not yet come to hand. EDIT.

 metals

metals compose the primitive matter of the globe, with more or less hydrogen, and still without the least portion of caloric. The acidifiable combustibles are dry acids and hydrogen; the salifiable combustibles, or metallo-fluores, are an acid and the metals; the acidifiable burners (*comburans*) are dry acids and oxygen; the common acids are dry acids and water; and the dry acids themselves are peculiar combustibles in which the hydrogen is saturated by at least double the quantity of oxygen that it is in water; which may combine with all the other bodies, but which cannot be put out of combination. All the other bodies are compounded of the latter. Water is decomposable by luminous caloric only: and when it oxidates bodies, it puts itself in the place of the equivalent of its contents in hydrogen, in the same way as, when hydrogen reduces bodies, it puts itself in the place of the water. I embrace the whole domain of chemistry in this manner: Will you have the goodness to submit my ideas to the penetration of men of science in your country?

"M. Dobereiner, of Jena, not having found any soda which contained iodine, sought for this substance in sea water; where he found it. The *intactile powder* is iodate hyperoxygenated by ammonia, as *detonating oil* is muriate hyperoxygenated with the same alkali. The iodate of ammonia may be formed by simple oxygenation, whereas the muriate of ammonia requires to be hyperoxygenated.

"You will be soon made acquainted with an experiment in which muriate of ammonia, obtained by the combination of its gaseous elements, deposited all the water from its acid, taking up in its stead muriate of mercury and *oxydule*. This fact is decisive for the existence of oxygen in chlorine, and triumphant for Mr. Murray.

"You will also find that the Prussic acid gas, and water and alcohol impregnated with this gas, kill in the most insignificant doses, and in three minutes, without convulsions, and as if a profound sleep had come on.

"I have ascertained that the essential oils which are distilled with alcohol or ether, cannot be again completely separated from those liquids, but retain at least the third of their weight; whether we attempt their precipitation by water, or try to make them float to the surface in the cold way.

"We have at Bruxelles a pile of Zamboni, which I have described in my French translation of Davy's Chemistry. It consists of disks of the diameter of a guinea, which are inclosed with pressure in two glass tubes of the form of columns. The substance of the disks is gilt paper sprinkled with native oxide of manganese, a vertical needle half a foot long, which is suspended about the sixth of its length towards the bottom, and oscillates

oscillates between the two columns, striking at each half oscillation two bells with which the columns are surmounted. This movement, which is not much different in point of rapidity from the pendulum of a clock of the same length, has now existed for seven months: it is a true perpetual motion arising from a physical impulse. The circulation ascends this pile dry, and no chemical composition exhausts it.

"There has been lately found in the calcareous stone of Chimaii, which is a blue bituminous shell-stone, a living toad of supernatural size. Has this animal been surprised in the formation of the stone, or has the stone generated it? It is conceivable that, by exclusion from the air, vitality might be only suspended, but then the substance of the stone must have nourished it to make it grow."

ACCOUNT OF SOME ELECTRICAL PHÆNOMENA

Recently communicated to the Royal Society of Edinburgh.

In the year 1767, M. de Saussure, M. Pictet, and M. Jalabert ascended to Mount Breven, which is situated nearly in the middle of the valley of Chamouni, and almost exactly opposite to Mount Blanc. Their object was to take a general view of the form and position of the glaciers which descend from Mount Blanc. When they reached the summit, M. Jalabert began to take a drawing of the glaciers, M. Pictet was engaged in the geographical part, and M. de Saussure was preparing to make his experiments on natural and artificial electricity. When M. Pictet was laying down upon his plan some particular mountains by means of a graphometer, he had occasion to ask the names of them from the guides, and was therefore obliged to point out the individual mountains with his finger. Every time that he raised his hand for that purpose, he felt at the extremity of his finger a kind of tremulous and prickly sensation, similar to that which is experienced upon presenting the finger to a globe of glass highly electrified. M. Pictet soon perceived the origin of this phænomenon. He observed a stormy cloud in the middle region of Mount Blanc, directly opposite to Mount Breven, and it instantly occurred to him that he had been affected with the electricity of the cloud. He then requested MM. de Saussure and Jalabert to make the same experiment; and as soon as they extended their hands, they experienced the same sensation, which they described as resembling a number of small electric sparks; but fearing that they might be seduced by their imagination, they made their guides and their servants stretch out their hand, and they felt the same sensation. The electricity of the atmosphere having now begun to increase, the sensation became stronger,

 and

and was even accompanied with a kind of whistling noise. M. Jalabert, who had a gold band upon his hat, was alarmed with a buzzing noise about his head; and having taken off his hat, and put it successively upon the heads of Pictet and Saussure, they heard distinctly the same sound, and even obtained sparks rom the golden button of the hat. The thunder cloud had now moved across the valley, and was directly above their heads. The thunder was loud, and the flashes of lightning so frequent, that the travellers found it prudent to descend about twenty or thirty yards, where no electricity could be perceived. The guides, however, were so much delighted with the experiments, that it was with great difficulty they were persuaded to abandon such a dangerous amusement. A shower of rain soon afterwards fell, and the storm ceased. The travellers re-ascended to the summit; and although they elevated an electrical kite, they were unable to perceive any indications of electricity in the atmosphere.

Electrical phænomena, much similar to the above, were very recently observed by a party of Englishmen, when they were descending Mount Ætna during a storm of thunder and lightning. The following particulars were communicated by Mr. Gillies, surgeon, who not only read an account of the phænomena in a journal entitled *Specchio della scienza e Giornale Encyclopedica de Sicilia*, which was published in July 1814, but received an account of them from Mr. Tupper, one of the party, coinciding in every respect with that which was given in the Sicilian journal. On the 2d of June, Mr. Tupper and Mr. Lanfiar, accompanied by a guide, ascended Mount Ætna.—During their descent, when at a little distance from the place called the English House, in the *regio deserta*, they were overtaken by a storm of thunder and lightning, accompanied with a heavy fall of snow; and while running over an extensive field of snow, they were struck by a flash of lightning, from which, however, they experienced no serious injury. Mr. Tupper felt a painful sensation in his back, which gradually ascended towards his head, and occasioned a sensation as if his hair was moving, an effect exactly similar to that which is produced either when a person is electrified upon an insulated support, or when his head is presented to a prime conductor of an electrifying machine. This sensation induced Mr. Tupper to raise his hand to his head, upon which he was surprised to hear a buzzing noise proceeding from his finger. Upon raising and extending his arm the noise still continued; but upon moving his hand and fingers in different directions, and with different degrees of velocity, he found that he could produce, at pleasure, a great variety of harmonic sounds, differing in their tone as well as in their

their intensity, and so loud as that they could be distinctly heard at the distance of forty feet. The Sicilian guide witnessed these phænomena with extreme dismay; and imagining that Mr. Tupper produced the sounds in virtue of some supernatural power, he immediately began to cross himself, and invoked the protection of his saint. His alarm, however, speedily subsided, when, upon being desired to elevate his own arm, he found it as musical as that of Mr. Tupper. Mr. Lanfiar, who was a little behind the rest of the party, now joined them, and found that his fingers possessed a similar property. In the course of five minutes, reckoning we presume from the arrival of Mr. Lanfiar, their fingers lost their acoustic property, the cloud having by this time passed to a considerable distance. Mr. Tupper had received an injury in his left shoulder joint by a fall from his horse; but he never experienced any return of the pain after the copious electrization which he received on Mount Ætna.

The preceding phænomena admit of a ready explanation, upon the simplest principles of electricity. As snow is a conductor next in order to water, and very little inferior to it in its power of transmitting electricity, it is quite obvious that the bodies of the travellers were not overcharged with the electric fluid in consequence of any difficulty which it experienced on passing into the earth. Their fingers therefore acted like so many points in drawing electricity from an atmosphere highly charged, just in the same manner as when the hand or the head is presented to an electrified prime conductor. The variety in the character, as well as in the intensity, of the sounds which were produced at the points of their fingers, arose from the different velocities of their fingers, and may be readily imitated by any other species of sound. The buzzing noise which M. Jalabert heard round his head had a different origin, having been produced by the discharge of the electric matter which had been accumulated in the gold band, and which found a readier escape into the atmosphere by the numerous points of gold thread, than by the imperfect conducting power of the hat which it encircled.

SOCIETY FOR PREVENTING ACCIDENTS IN COAL-MINES.

A society under the above denomination was established in Sunderland in 1813, which has for its objects to ascertain with more precision than has hitherto been effected, the causes of those explosions which so frequently occur in coal mines, producing extensive and deplorable calamities; and the measures which may be best calculated to prevent them.

The first Report of the Society was published a few weeks ago. The Committee state with regret, " that hitherto no suggestion

has pointed out any adequate mode of destroying, or of preventing the generation of, the inflammable gas; or of so completely ventilating the pits, as to secure them from its dreadful effects. They are not, therefore, in possession of sufficient information, fully and exactly to specify all the circumstances which are necessary to be attended to, in promoting the discovery of any general measures of correction for the evils lamented: and they are compelled to add, that they must look to a more extensive support than they have hitherto received, to enable them to hold out such encouragement to scientific and practical men, as may stimulate their attention to the subject: for, notwithstanding the general approbation which their designs have obtained, and the liberal subscription which they have received from the noble and respectable individuals who have countenanced the Society, their funds do not yet impower them to offer a premium, suitable to the object, for the best production that may be procured. They still, however, flatter themselves, that as their proceedings shall be further disclosed, they will obtain a more ample support, which may give effect to their views."

The Committee have published in their Report a valuable communication made to the Society by Mr. Buddle, a gentleman of great celebrity and intelligence as a viewer of coal mines, detailing the means adopted in some extensive collieries under his inspection.

The only method at present followed to prevent accidents by explosion is by ventilation of the several passages and workings of the mine—that is, a mechanical application of the atmospheric air to remove or sweep away the inflammable gas as it is generated in the workings, or as it issues from the several fissures which the workings intersect in their progress. To assist in this operation a furnace is sometimes employed in the pit, supplied with atmospheric air drawn down one shaft, and made to circulate through different windings till it reaches the ascending shaft. In other cases ventilation is maintained by means of a horizontal hot cylinder fixed in a furnace of brick-work on the surface. The cylinder (any old one of a steam engine) being completely enveloped in flame, by rarefaction induces the air of the mine in a regular current up the shaft, with which one end of it is made to communicate by means of a flue, and discharges it into another flue at the other end of the cylinder. The shaft of course is covered over, that the current through the cylinder may not be disturbed in its direction. The flue at the outer end of the tube is for the purpose of discharging the inflammable air at such a distance from the furnace as may prevent any possibility of its being inflamed. It is a curious fact, that

that hydrogen gas is never inflamed at hot iron. Another mean resorted to for ventilation is an air-pump on a large scale, wrought by a steam engine. It is made of three-inch plank, with a piston five feet square: the stroke is eight feet long; and the suction pipe and valves about one-third of the area of the piston. At twenty strokes per minute it will draw 8000 cubic feet or 778 hogsheads of air in that time; but allowing a waste of one-fourth it will draw 584 hogsheads per minute.

Mr. Buddle's paper is accompanied with ten engravings illustrating the apparatus, and the methods employed to direct the course of the current of air in the workings, which may be compared to alleys crossing each other at right angles. By a judicious arrangement some of these are closed at particular intersections, while others are kept open. We cannot doubt that this communication will prove of great service to mine viewers. It is written with great perspicuity, and by means of the engravings is rendered very intelligible.

We cannot close this notice without an expression of regret that there should be any want of friends to enable the Society to prosecute with desirable efficacy the great object of their association. We confess that we are surprised at the fact, when we consider how many rich individuals there are who are so deeply interested in the result of the Society's labours—not in Sunderland only but in other counties. Let us hope that ample means will soon be provided, and that the publicity which the Society gives to its labours will be a mean of turning the attention of men of science to a subject of so much importance.

STATE OF MEDICINE IN CHINA.

M. Page, a physician of Orleans in France, has published a work on this subject. The following short account of the Chinese medical practice will probably amuse some of our readers:

"The Chinese employ emetics and purgatives, but very rarely: clysters are almost never used, because they regard them as too European, but they make a free use of cordials. The importation of opium is prohibited under pain of death.

"The Chinese in the treatment of the itch and eruptive diseases employ camphor and cinnabar also, with sulphur dissolved in woman's milk. They make use of borax in inflammations of the throat; it is reduced into powder, and blown upon the diseased part. They borrowed the use of the bark from the Jesuit missionaries.

"They were acquainted with inoculation long before us. They practise it in general by introducing into one of the nostrils cotton imbibed with variolous matter; the cotton is allowed to remain

remain twelve hours, and in seven days at latest the disease appears.

"Like most Indian nations, they make a free use of aphrodisiacs, baths and mineral waters. They have springs saturated with alum and iron, but the greater number contain sulphur. Their physicians are not able to analyse them. Chemistry as well as natural history is in its infancy in China. But the Chinese have the good fortune to possess a species of *mesmerism* or animal magnetism, as practised by certain sects of illuminati in Germany. The Chinese literati strive to put down this sect by ridicule; but they nevertheless find proselytes daily, to what they are pleased to call the *science of sciences*.

"The Chinese are not acquainted with the making of bread, for which they substitute boiled rice or maize: their wine is a strong liquor extracted from honey or fermented rice. They do not drink either coffee or chocolate—they have delicious melons, the species of which is unknown to us, some very delicate kinds of small onions, and several delicious plants; but they have no olives, strawberries, gooseberries, or potatoes.

"The diseases of stone and gravel are wholly unknown to the Chinese—in consequence, as they tell us, of the great quantity of tea which they drink *.

TANNIN.

M. Pelletier has published in the *Annales de Chimie* some observations with a view to show the imperfect state of our knowledge of this substance and its combinations with gallic acid. The various kinds of tannin which have been successively produced from various processes, are different in the greater part of their properties: they have nothing indeed in common, but the property of several animal substances, and forming with them insoluble combinations which are not susceptible of putrefaction, and have also the power of precipitating in a manner nearly similar even metallic solutions; but they are different in their taste, colour, solubility in water, &c. Pure tannin does not exist: the properties which are attributed to it, and by which it is characterized, belong to several combinations which vegetable substances form. Why then, asks M. Pelletier, shall we continue to consider this as a distinct principle?

Is it because it precipitates several metallic oxides from their solution? Almost all vegetable extracts have the same property,

* The great consumption of tea in England is well known: are the affections arising from urinary calculi less frequent in that country? We may form some idea on this subject by reading the papers of Brande, Home, and Hatchett.—*Note by the French Editors of the Annales de Chimie.*

and

and we know that these extracts are at least triple combinations of acid, the colouring substance, and of vegeto-animal matter, because the precipitates which form the tannin matter in those solutions are constantly coloured and sometimes very brilliant. But if we reflect that gallic acid always accompanies tannin, and that the colours of the precipitates furnished by the tanning matter, and the metallic solutions, are the same with those manifested by the addition of the gallic acid, and the same metallic solutions; may we not conclude with M. Thenard, that the colouring of these precipitates is owing to the gallic acid, from which we can never entirely separate tannin? or is it the property which tannin has of combining with animal matter and preserving it from putridity? A multiplicity of combinations of vegetable matter also possess this property; and without mentioning the astringent matter formed by the action of mineral acids on charcoal and several vegetable substances, or the experiments of M. Chevreul on *hematine* (which acquired this property) and who disbelieves the existence of tannin? I shall mention some facts to prove that gallic acid can combine with several vegetable substances, and thus acquire the properties of tannin. If we put a solution of pure gelatine in gallic acid, no precipitate is formed: this acid does not produce any turbidness in the gummy solutions, but they cannot be resolved without immediately becoming turbid in white flakes which are soon precipitated. Among the pharmaceutical extracts there is a great number which do not contain the astringent principle, and which form no precipitate in the solution of gelatine; but by the addition of gallic acid they acquire this property. The same phænomenon does not take place with the other vegetable acids, which on the contrary seem to oppose themselves to the precipitation of gelatine.

We know that pure gallic acid forms no precipitate in a solution of sulphate of iron at the maximum, but it there becomes a beautiful deep-blue colour. Infusion of nut-galls produces, on the contrary, a precipitate which is attributed to tannin; but gallic acid of itself acquires the property of partly precipitating the iron from this solution when it is combined with extractive matter: most of the vegetable infusions unite with gallic acid and gelatine, the same as the extractive substances, for which we can assign no reason. The phænomenon is very perceptible with the cold infusion of saffron: the properties of these precipitates cannot be absolutely identical; they must differ according to the nature of the substances which enter into each combination: that formed by gum arabic, gelatine and gallic acid is the only one which I have hitherto been able to examine: it differs from the others by its extreme adherence to water, with which

which it assumes an oleaginous form, and partly passes through the paper filter. This combination, it appears, may exist in different proportions, which I have not been able yet to determine; except that it does not putrefy: whereas in the others, a fetid smell arises in a few days, and much later than if the gelatine was pure. The combinations of the gallic acid with gelatine and extractive matter unite less freely with water, and resemble much the precipitates formed by the infusion of nut-galls in the gelatinous solution.

A CAUTION TO CHEMISTS.

M. Scharinger, an eminent chemist of Vienna, died lately, from the fatal effects of a chemical experiment. While preparing the prussic acid (*acidum borassicum*) and the most powerful poison known, he spilled a considerable quantity on his naked arm, and died in a few hours in great agonies.

VERDIGRIS.

Señor Orpila has found that great quantities of dry sugar taken into the stomach of a patient who had recently swallowed verdigris, or eaten food prepared in untinned copper vessels, have proved an immediate and effectual antidote to the cupreous poison. In cases where the poison has remained so long in the stomach as to produce inflammation, the usual remedies for the latter must be conjoined with the sugar.

DETONATING OIL.

The original discovery of this substance, which has been claimed by M. Dulong, and actually discovered by Davy, appears to belong to M. Van Mons of Brussels. This distinguished chemist, whose discoveries have often been appropriated by others, observed in 1793 the combination of ammonia with oxygenated muriatic acid. Nineteen years before the experiments of Dulong or Davy, he announced his discovery to Gren, in the following terms, dated Messidor, An 4 (1796): "I have succeeded in combining ammonia with oxymuriatic acid without any decomposition whatever of these two bodies. This new salt detonates at a certain degree of heat, whether in the open air, under water, or under other liquids by which it is not decomposed*." The same facts were stated in Dandolo's Italian translation of Van Mons's Chemical Philosophy, "Il muriato ossigenato d'ammoniaco si reduce à suoì principi e detona à con-

* "Es ist mer gelurgen das ammiak mit der oxigenirten salzsaure zu verbinden ohne dass dabey eine zersetzung dieser substanzen vorgegangen waere. Dieses neues zalz detonirt beg einem gewissen grad de waerme, sowohl in freyer luft als unter wasser und anderen tropfbaren flussigkeiten die es nicht zersetzen." *Neues Journal der Physik.* Bd. 3, s. 230.

forza

forza anche sotto l'acqua, colla semplice impressione del calore." Van Mons's *Mémoire*, &c. to the Royal Academy of Sweden, p. 224–5.

LIST OF PATENTS FOR NEW INVENTIONS.

To Robert Dickinson, Esq. of Great Queen Street, Middlesex, for his improvements in the art of sadlery.—28 Nov. 1814.—6 months.

To Robert Dickinson, Esq. of Great Queen Street, Middlesex, for his improvements in the manufacture of barrels and other packages made of iron or other metals.—10 Dec.—6 months.

To Robert Salmon, of Woburn, Bedfordshire, surveyor, for his improved movements and combinations of wheels for working of cranes, mills, and all sorts of machinery, either portable or fixed.—10 Dec.—2 months.

To Edward Glover, of Penton Place, Walworth, surveyor, for his apparatus for drawing or extracting bolts, nails, &c. and for various other useful purposes.—10 Dec.—6 months.

To Henry Julius Winter, of Dover, Kent, confectioner, for his method of giving effect to various operating processes.—12 Dec.—6 months.

To Joseph C. Dyer, of Bolton, state of Massachusetts, now residing at Gloucester Place, Camden Town, Middlesex, merchant, who, in consequence of improvements by himself, and of a communication made to him by a foreigner residing abroad, is in possession of certain additions to and improvements on machinery, to be made and applied in manufacturing cards for carding wool, cotton, silk, tow, and other fibrous materials of the like description.—15 Dec.—6 months.

To John Francis Wyatt, of Furnival's Inn, engineer, for his new kind of bricks or blocks, one of which is particularly adapted for the fronts of houses and other buildings, giving to them the appearance of stone; another is applicable to a new method of bonding brickwork; also a new kind of blocks or slabs for paving floors and facing or lining walls instead of ashler, which will resemble marble or stone, and which may also be applied to steps or stairs, and other parts of buildings.—15 Dec.—2 months.

To William Everhard, Baron Von Doornick, of Sun Street, Bishopsgate Street, London, for his improvement in the manufacture of soap.—20 Dec.—6 months.

To James Smith, of Newark upon Trent, Nottingham, cabinet maker, for his improved self-acting sash fastening.—20 Dec.—2 months.

To Robert Dickinson, Esq. of Great Queen Street, Middle-

sex,

sex, for certain improvements in implements applicable to the Ships Nun Buoy and Bacon Buoy.—20 Dec.—6 months.

To John Vallance, jun. of Brighthelmston, Sussex, brewer, for his apparatus and method of so constructing and securing brewers' vats or store casks, as to prevent the vats falling to pieces, or even breaking, though every one of the hoops on it should be broken in sunder, and consequently preventing any beer from being lost; and also for preventing the loss of any beer, even if a cock, or if all the cocks of the vat should be broken off.—20 Dec.—6 months.

To Frederick Koenig, of Castle Street, Finsbury Square, in the county of Middlesex, printer, for his invented certain further improvements on his method of printing by means of machinery.—24 Dec.—18 months.

To Edward Jordan, of Norwich, engineer, and William Cooke, of the same place, machine maker, for their apparatus for the detection of depredators, which they denominate The thieves' alarm.—24th Dec.—6 months.

To John White, of New Compton Street, Soho, Middlesex, for a new and improved method of making candles.—27th Dec.—2 months.

To Joseph Harris, of Shire Lane, in the liberty of the Rolls, Middlesex, army accoutrement maker, for his improvements in necessaries or clothing used for the military in general.—4th Jan. 1815.—6 months.

To Christopher Dihl, of Brewer Street, Golden Square, for his means of making a mastic cement or composition which he denominates Dihl's Mastic.—6th Jan.—15 months.

To John Cutler, of Great Queen Street, Lincoln's Inn Fields, iron founder, for certain improvements applicable to fire-places, stoves, &c.—6th Jan.—6 months.

Meteorological Observations made at Walthamstow, in Essex, from December 1 *to* 18, 1814. *Communicated by* Thomas Forster, *Esq.*

Dec. 1.—Thermometer 42° and 30°. Barometer 29·44. Clear and cloudless, and wind N.

Dec. 2.—Thermometer 38° and 32°. Barometer 29·80. Snow lying on *dung* and *wood*, not on the ground, fallen in the night *past*. Gray day; very dark night. N.

Dec. 3.—Thermometer 36° and 28°. Barometer 29·80. Foggy. Gray day. N.

Dec. 4.—Thermometer 42° and 34°. Barometer 29·40. Heavy rain—sunshine. S.

Dec.

Dec. 5.—Thermometer 42° and 32°. Barometer 29·30. Showers and wind. N.W.

Dec. 6.—Thermometer 38° and 32°. Barometer 29·90. Fair day. N.W.

Dec. 7.—Thermometer 44° and 40°. Barometer 30·00. Sleet; showers—dark night. N.

Dec. 8.—Thermometer 52° and 47°. Barometer 29·31. Showers and high wind; starlight. S.W.

Dec. 9.—Thermometer 52° and 26°. Barometer 29·30. Showers and wind. S.W—N.

Dec. 10.—Thermometer 51° and 48°. Barometer 29·70. White frost, showers and wind. N.

Dec. 11.—Thermometer 52° and 47°. Barometer 29·39. Showery—very dark night. S.W.

Dec. 12.—Thermometer 57° and 54°. Barometer 29·70. Showers and wind—rainy; very dark. S.W.

Dec. 13.—Thermometer 57° and 42°. Barometer 29·68. Showers and sun—star-light night. S.W.

Dec. 14.—Thermometer 52° and 59°. Barometer 29·76. Showers and wind—very stormy. S.W.

Dec. 15.—Thermometer 52° and 45°. Barometer 29·50. Clear and cold; sleet; rain—starlight. S.W.

Dec. 16.—Thermometer 55° and 39°. Barometer 29·76. Stormy; rain and hail—starlight. S.W.

Dec. 17.—Thermometer 56° and 53°. Barometer 30·00. Showers and sun—dark and wind. S.W.

Dec. 18.—Thermometer 51° and 49°. Barometer 29·70. Sun and wind—cloudy. S.W.

Walthamstow,
December 18, 1814.

8. D. D.

During the period comprehended in the above observations, a remarkable changeableness characterized the weather; the sudden vicissitude of frost and rain, cold and warmth; all sometimes occurring within the twenty-four hours, afforded a striking specimen of the changeableness of our climate.

Not having had an opportunity of daily noting down the weather myself, I give instead the above journal, made by a relation at Walthamstow, in Essex, about six miles N.E. of St. Paul's.

Clapton,
Dec. 19, 1814.

THOMAS FORSTER.

METEORO-

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand,

For January 1815.

Days of Month.		Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Dec.	27	35	40	41	29·09	0	Rain and Snow
	28	42	38	37	·15	0	Rain
	29	37	42	43	·75	0	Rain
	30	45	49	40	·87	0	Rain
	31	37	44	41	·95	7	Fair
Jan.	1	36	41	35	30·22	10	Fair
	2	34	35	33	·45	0	Foggy
	3	33	34	32	·42	0	Foggy
	4	30	34	29	·13	0	Cloudy
	5	29	33	29	·14	0	Cloudy
	6	29	36	30	·14	0	Cloudy
	7	27	36	36	29·84	5	Fair
	8	32	35	29	·52	11	Fair
	9	27	38	40	30·02	6	Fair
	10	41	43	36	29·80	7	Fair
	11	34	37	35	·92	9	Fair
	12	31	38	30	·98	8	Fair
	13	27	35	40	·92	0	Rain
	14	40	40	35	·90	0	Cloudy
	15	34	35	29	30·20	7	Cloudy
	16	26	30	30	·16	0	Cloudy
	17	35	39	29	·30	0	Cloudy
	18	29	33	30	·15	0	Snow
	19	26	33	24	29·87	0	Showers of Snow
	20	27	28	27	·72	0	Snow
	21	27	32	28	·74	0	Snow
	22	28	32	29	·80	0	Snow
	23	29	30	25	·76	0	Showers of Snow
	24	19	27	28	·66	0	Foggy
	25	25	28	27	·60	0	Cloudy
	26	26	30	25	·28	0	Cloudy

N.B. The Barometer's height is taken at one o'clock.

XVII. *Observations on the Geology of Northumberland and Durham: and Remarks on Mr.* WESTGARTH FORSTER'S *Section of the Strata, with a Sketch of the physical Structure of that Part of England, from the German Ocean to the Irish Channel. By* ROBERT BAKEWELL, *Esq.*

To Mr. Tilloch.

SIR,—I PROMISED to send you some observations on the geology of the north-eastern part of England, which various circumstances have prevented me from performing, and the following account may now perhaps be considered superfluous Dr. Thomson having recently published what he denominates A geognostical Sketch of the Counties of Northumberland and Durham. Such, however, is the imperfect state of our information on various subjects of geological inquiry in our island, that it cannot be deemed presumptuous in any one to suppose that he may add something to what was before known; and I trust the present letter will not be construed into an attempt to disparage or undervalue the observations of those who have preceded me:—nothing can be more remote from my intention. In proportion to the increase of observers will be the probability of attaining certainty in our conclusions. In a letter I received from Dr. Watson, the venerable Bishop of Llandaff, on the publication of my Introduction to Geology in 1813, this sentiment is so clearly stated, and is so applicable to the present subject, that I shall be excused for transcribing it.

"The surface of the globe consists of three parts water and but one of earth, yet the accurate delineation of the one part would occupy the labours of all the philosophers in Europe for fifty years. Nothing less than such a delineation can ascertain the connections, interruptions, and mutual dependencies of the several strata which compose its surface. Some are apt to inquire, What is the use of such investigations? I esteem these men to be as simple in their notions as the academic youths, who being puzzled in attempting to pass the Ass's bridge in Euclid, ask, Where is the use of going over it? For my own part, I am so confident of the utility that would attend an accurate knowledge of the stratification of this island, that I think a work of this kind ought to be undertaken at the public expense. The beds and veins of metallic ores, and the beds of coal, limestone, marble, slate, &c. are of such high importance in the present state of society, that every encouragement should be given to the discovering of them where they are not at present known; and nothing can more contribute to this end, than an accurate knowledge of the manner in which the strata appear to

 the

the day in mountainous countries. Analogy would then enable us to discover them where they are not known at present."

I visited the coal districts of Northumberland and Durham in the summer of 1813, and had frequent communications with several of the most intelligent coal proprietors and agents, and with gentlemen who were interested in geological inquiries. Much has been already done to elucidate the geology of part of these districts by Mr. Westgarth Forster, whose section of the strata taken in great part from actual admeasurement comprises the coal districts on the eastern and middle parts, and the metalliferous limestone districts on the west, an extent of forty miles, and an actual depth of nearly 1400 yards. This section, perhaps the most important and extensive that has ever been made, was taken by a person educated as a practical miner, who had spent the early part of his life in that district; as such I consider it particularly valuable. It was published in 1809. I am a little surprised it has escaped Dr. Thomson's notice when describing the geology of Northumberland and Durham. If the present paper have no other merit than that of making Mr. Forster's section more generally known, it will render an acceptable service to English geologists, and do some justice to a person whose labours have not been sufficiently appretiated.

The mineralogical district to which the present observations relate, is bounded by the river Tees and the Cleveland Hills on the south, by the German Ocean on the east, by the porphyry and amygdaloid of the Cheviot Hills on the north, and by the mountain of slate porphyry, gray-wacke, and sienite of Cumberland, on the west. The general arrangement of the strata in Northumberland and Durham is such as prevails on the eastern side of England, as represented in the map prefixed to my Introduction to Geology, namely, the upper calcareous strata with magnesian limestone on the east, the strata containing coal range through the middle part, and the metalliferous limestone districts rise to the west. The strata on this side of England generally dip towards the east, and rise gradually towards the west or south-west, when the descent becomes abrupt; which is also the case in Yorkshire, Derbyshire, and the north part of Staffordshire. We may consider this district as divided into three parts, consisting, 1st, of calcareous strata with magnesian limestone; 2dly, of the coal strata; and 3dly, of the lower metalliferous strata.

The first, or magnesian limestone district, on the east, was not described or measured by Mr. W. Forster, as it contains neither metallic ores nor coal*. It presents, however, many features particularly interesting to the mineralogist, of which I shall endeavour

* This must be taken with some limitation, as we shall presently have to observe.

deavour to give a brief account from my own observations. It may be proper to remark previously, that the strata in these counties are frequently broken and deranged by numerous nearly perpendicular walls or dykes both of clay and whin-stone (basalt) of vast extent; besides which, numerous blocks of whin-stone, of a different kind from any which these dykes furnish, are scattered over the surface of the country.

The calcareous strata and magnesian lime, which for the sake of conciseness I will denominate the Sunderland limestone, (being there most extensively quarried,) is lost on the south-east under the alluvial ground of the river Tees, and cut off in that direction by the lofty range of the Cleveland Hills: but the same formation makes its appearance on the south-western side of these hills, and is continued through Yorkshire into Nottinghamshire and Derbyshire and through the south-eastern counties. The chalk and roestone, which are the upper series of these strata in other situations, are stripped off, if they have ever covered the magnesian limestone of Northumberland and Durham.

The Sunderland limestone formation extends along the coast north of the Tyne, but not in a continued line. The whole thickness of this limestone has not been measured; nor would it be easy to acertain it, as some of the rocks are very indistinctly stratified. I think it cannot be less than one hundred and fifty yards. Two hills on the west of Sunderland, containing numerous marine organic remains, I am inclined to consider as of subsequent formation.

On the banks of the Wear, about two miles west of the Iron Bridge, the lower beds of the Sunderland limestone are very extensively quarried to the depth of at least one hundred and thirty feet from the surface: it is here most distinctly stratified, rising in large tables and slabs well suited for building stone, being compact and extremely durable. In one instance, and in one only that I could hear of, have any organic remains been found in the limestone of this quarry. The perfect impression of the head and vertebræ of a flat fish about seven inches in length, was discovered in dividing a slab of this stone: one side of the impression was presented to the Sunderland Museum; the other is in the possession of J. Goodchild, Esq. the proprietor of the quarry. I am more inclined to believe that the rare occurrence of organic remains in these rocks is to be attributed to some process of nature by which they have been obliterated, than to conclude that they never were imbedded in them.

Near the quarry I have just mentioned, an attempt was lately made by Mr. Goodchild to search for coal by boring through the limestone. It was attended with much expense, but with

complete success. After perforating the whole of the subjacent limestone rock, they bored through a lower bed, which Mr. G. described to me as a dark-coloured argillaceous shale with occasional hard stones which he considered as loose whinstones. I have unfortunately mislaid the note respecting the absolute depth of this bed given me by Mr. G., but I think it was not less than 130 yards, and the whole depth of the upper good workable coal not less than 260 yards. At a greater depth under this was another valuable coal bed. I mention this fact, as it is of some geological importance, and is, I believe, the only instance in the district of coal having been actually found by boring through the magnesian limestone. Some of the mining agents at Newcastle, who were unacquainted with the above circumstance, gave me their decided opinion that the coal *cropped* out, or was cut off by dykes, before it reached the limestone of Sunderland. It may perhaps deserve future inquiry, whether this thick bed of shale above mentioned be not a continuation of the aluminous schistus from the Cleveland Hills on the south.

The limestone of Building Hill near Sunderland and at Fulwell is particularly deserving notice, from the remarkable configurations which it presents.

The beds at the former place are of considerable thickness: the limestone is imperfectly crystalline, is of a yellowish-brown colour, and yields a fœtid smell when struck with a hammer, being that species called by mineralogists swinestone. It contains nearly the same proportion of magnesia as that of Breedon in Leicestershire, first analysed by Mr. Tennant.

The limestone in some of the beds is divided into small cells uniting with each other, and pretty regularly arranged: this has received the appellation of honeycomb limestone, a name which conveys a tolerably correct idea of its appearance. A superficial observer might suppose from the form that this was the organic remains of some species of madrepores; but it is evidently the result of a tendency to crystalline arrangement. This arrangement has proceeded still further, and disposed distinct masses of the honeycomb limestone to assume determinate forms in the substance of the rock itself. The crystallization of some of these masses appears to have diverged from a centre laterally, until the radii were met by those of other diverging masses, and both became compressed on the sides. The most striking of these forms nearly resembles a papal mitre: they are from six to nine inches in length or more, and may be detached from the rock. Several of these mitre-shaped masses appear also to have come in contact during their formation, and to be compressed at the place of junction, the convexity of theone having

formed

formed a concavity in the sides of the other. We must either admit, with the late Mr. Gregory Watt, that a crystalline arrangement of the particles can take place in solid bodies at the common temperature of the earth, or that these rocks have been softened by heat, or some other cause, subsequently to their formation. Nor will even the latter opinion, I conceive, be very improbable; for on the elevated ground at a little distance to the north-west of Building Hill, I observed immense blocks of extremely hard and very black basalt lying on the surface of a ploughed field. On inquiry, the men who were working informed me that numerous blocks of the same kind were buried under the soil, which impeded their operations in ploughing so much, that, when they were too large to be removed, they were obliged to break them by blasting with gunpowder. I therefore think it extremely probable, that an immense whin dyke has intersected this formation of limestone. The quarry at Fulwell is close by the turnpike road to Newcastle, and about two miles from Sunderland. The limestone here presents little of the honeycomb appearance of that at Building Hill; but it is not the less remarkable: it is covered by a stratum of calcareous marle, or rather sand; some of the beds are also divided by the same pulverulent marle or calcareous sand, in which are imbedded numerous detached spheres, spheroids, and also botryoidal and stalactitical masses of limetone: but the latter are attached to the rock. The upper stratum of calcareous sand also contains numerous balls and clusters of balls of a similar kind, varying in size from that of a pea to ten or twelve inches in diameter. That these balls are not water-worn is most evident: many of them have a crystalline diverging radiated structure, others are curvedly lamellar. The most striking circumstance attending these balls remains to be noticed: many of them appear to have come in contact at a certain stage of their formation: they present the appearance of two or more balls with a segment cut from each, and closely united at the place of section; in other instances a number of balls appear to have pressed laterally on each other and flattened the sides, thus forming polyhedral prisms convex at their upper and lower extremities. This singular conformation had not escaped the attention of that sagacious young philosopher the late Mr. G. Watt. It is indeed a fact analogous to what took place in the experiment he made by melting seven cwt. of basalt, and suffering it to cool slowly. A number of small globules formed in the mass, and enlarged till they compressed each other into a prismatic shape. The formation of these balls of limestone in the soft strata of this rock would, I conceive, if properly attended to, throw some

light on the globular and columnar structure common to many rock formations.

Nearer to Sunderland, below the garden of the rectory, there is another remarkable quarry. If the limestone here once existed in continuous beds, it has been subsequently broken and thrown into the utmost confusion; large shapeless masses being piled together, and the intermediate spaces filled with calcareous sand similar to that before mentioned at Fulwell.

The limestone of this district, though abounding in magnesia, is sent in prodigious quantities to Scotland for agricultural use; which offers a further confirmation of what I have stated before in my Introduction to Geology, that magnesian limestone, so far from being prejudicial to land, is preferable to any other where it is to be sent to a distance; for the same quantity will produce an equal effect to a larger quantity of the common kind*.

The coal district of Northumberland and Durham is bounded by the magnesian limestone, or by the sea, on the east, and by the metalliferous limestone on the west. Dr. Thomson has divided this district into two formations: the one he calls the Newcastle coal formation; and the other the independent coal formation: but for this division there does not appear to me any sufficient reason, nor would it be easy, or indeed possible, to separate these formations by any well-defined characters; for the former is only the upper series of strata to the latter. The circumstance of galena veins occasionally occurring in the lower series will not entitle it to be considered as a distinct formation, by any one practically acquainted with the coal district of our island south of the Tweed.

When coal strata approach the metalliferous limestone, very strong veins of galena will be found occasionally to penetrate the coal, or to send up branches or strings of ore; the more powerful veins generally rising the highest into the upper strata. Similar instances might be cited in Derbyshire, Staffordshire, Lancashire, and other parts of England. As the whole of this district has been mined, and the succession and thickness of the strata ascertained, I shall proceed to give an account of Mr. Forster's section, which includes, as before stated, a measurement of nearly fourteen hundred yards. But as the earth has nowhere been perforated to that depth in any one situation, persons not familiar

* It is only necessary to use this lime more sparingly. Inattention to this circumstance gave rise to the opinion of its prejudicial effects on land. I was informed by a farmer in Derbyshire, that though no vegetation will spring up for two years, where a heap of the burnt magnesian limestone has been laid, after that time the place is always covered with an abundant and vigorous crop of white clover.

with

with such inquiries may find it difficult to conceive how this measurement was effected. I shall therefore add an explanatory sketch, which will, I trust, make the subject intelligible. By extending the line east and west beyond Mr. Forster's line of section, it will cross the whole island in that parallel of latitude, presenting a section of England which I believe will convey a tolerably correct idea of the geological arrangement of the rocks and strata from the German ocean to the Irish sea. The general dip of the strata on the eastern side of the island, as before stated, is to the south-east: and of course each stratum rises to the surface in the south-west, when not cut off by a fault.

The line LL is supposed to represent the level of the sea from which the beds of magnesian limestone rise on the east marked A. The coal formation commences at B, rising from under the magnesian stone. A perforation at 1 will measure the thickness of all the strata to the bed of coal marked *a*. By proceeding further west, the bed *a* rises near the surface at station 2, where another pit may be supposed to sink to stratum *b*. The depth of this added to the former will give the thickness of the whole strata from B to stratum *b* 2. Proceeding in a similar manner at station 3, we obtain the thickness of all the strata as far as C and C, on the west of the mountain at C. Here the strata making their appearance may be measured on the surface, due allowance being made for the angle of inclination. The section of Mr. F. begins west of the magnesian limestone. The first bed is the clay and soil on the surface, under which is a bed of hard siliceous sandstone called provincially *post*, and at the depth of seventeen fathoms the first thin seam of coal, under which the various strata of slate clay or shale. Bituminous shale and variously coloured sandstones succeed each other; the most remarkable of which are designated by their provincial names, and those of minor importance by the letters *a* or *s*, to denote that they are principally argillaceous or siliceous. An inspection of the plate will render any further explanation unnecessary, besides that of the provincial names which are given below.

More than thirty beds or seams of coal succeed each other, which are sufficiently well marked to be distinguished in different situations where they occur; each bed of coal generally preserving its own strata above and below it in its whole extent. It may be observed that all the principal coal beds are comprised within 200 fathoms, or 400 yards, from the beginning of the section. Though some few beds occur in the lower strata, they are of inferior quality and generally not workable. In a similar manner in the Yorkshire and Derbyshire coal fields, there are a few thin seams of coal in the great mass of sandstone and shale which lie under the principal coal beds, between them and the metal-

 liferous

liferous limestone. The most valuable coal beds are called the High main coal and the Low main coal, which are six feet or upwards in thickness on the Tyne; but on the Wear, if they be the same strata, their thickness is considerably less. Each stratum of coal generally, but not invariably, preserves nearly the same degree of thickness throughout its whole extent: but there are instances here, as well as in Staffordshire and Yorkshire, of a bed of coal dividing into two parts, having different degrees of inclination; or, perhaps, to speak more correctly, two beds of coal in descending meet and unite. The separating strata having disappeared in such instances, the bed will be much thicker than before the junction.

The other strata also vary in thickness in different parts of their course, but the limestone strata are observed to be more uniformly regular than the rest. The very hard strata which are obliged to be sunk through by blasting, are called whinstone by the miners; but these are not the proper whinstone, or basalt, similar to that of the dykes. There is however one bed of true whinstone of vast thickness called the great whinstone (sill) or bed, which is found to vary from 16 to 60 yards in tkickness. This bed lies far below the principal coal strata. It is considered by Mr. Forster as the same which appears on the banks of the river Tees, where it forms magnificent basaltic columns, some of which are not less than 40 feet in height. It extends to the mountain called Cross Fell, on one side of which, I am informed, there is a deep nearly circular excavation called High Cup, surrounded by lofty basaltic columns. An intelligent gentleman well skilled in mineralogy, who has resided long in the county, and paid particular attention to the subject, informed me that he did not consider the great whin sill as a regular stratum, but as a wedge-shaped mass of basalt, probably formed by the expansion of basalt from one of the great basalt dykes which intersect that district. The provincial word *post* signifies a very firm siliceous sandstone, and is differently named according to its colour, as gray post, white post, &c. Hazle is also a name given to some kinds of siliceous sandstone. Coal shale and the clay slate of Werner are called *plate*. Gray beds are series of thin alternating strata of siliceous sandstone and shale. It would be tedious to enumerate the provincial names of all the beds; it will be sufficient to mark them by the letters S and *a*, to denote their predominating characters, as siliceous or argillaceous strata. In the rooms belonging to the Philosophical and Literary Society at Newcastle, sections of some of the principal coal mines are kept, with series of mineral specimens of the different strata, and their provincial names affixed. It is deserving notice, that in some of the coal mines there are brine springs containing

containing about 5 per cent. of common salt. At Long Benton in Northumberland, the proprietors have obtained by act of parliament the exclusive privilege of extracting soda from the brine, without the payment of the regular duties. Another spring rises in part of the river Wear*.

At the depth of about 300 yards below the lowest principal beds of coal, the beds of metalliferous limestone commence, particularly what are provincially called the Little and the Great limestones. There is a bed below these sometimes called the Great limestone, or the Melmerby Scar limestone, which is much thicker than the upper great limestone bed. The Little limestone is the first of the metalliferous limestones. Between this and the first great limestone are strata of sandstone and plate, with two seams of sulphurous coal. The first great limestone is about 21 yards in thickness, and is the most productive of lead of any limestone on the Wear or Tees. About four yards of the upper parts of this limestone in Northumberland are called the Tumbler beds; they contain entrochi and other organic remains. There are in all not less than nineteen beds of limestone, of which the most remarkable are called the Four fathom limestone; the Five yards limestone; the Three yards limestone; Scar limestone (five fathoms); Tyne bottom limestone, so called because the river Tyne runs in this limestone nearly all the way from Tyne head to Garrygall gate, a distance of about four miles. It is stated by Mr. Forster to be the lowest limestone of Alston Moor, and the uppermost stratum at Dufton Fell in Westmoreland: its

* The difference in the thickness of the upper coal beds on the rivers Tyne and Wear may perhaps make it doubtful whether they are the same (according to Mr. Forster): "the first eight small seams and the high main coal on the river Tyne are scarcely noticed on the river Wear. The next below called the metal coal on Tyne, makes part of the five quarters coal on Wear; then the next called the yard coal on Tyne forms the high main coal on Wear six feet thick; then there is a small seam six inches thick, from which we come to the Bentham seam on Tyne three feet three inches thick, which forms the Maudlin seam on the Wear four feet thick. The next below is a coal called the six quarter coal on Tyne three feet six inches thick, part of the low main coal on Wear. Underneath is a small seam nine inches thick, from whence we come to the low main coal on Tyne six feet six inches thick, which forms the Hutton seam on Wear four feet three inches thick. From thence we find five seams, as will appear by the section, when we come to the Hervey seam three feet thick on Tyne, and to Wickham stone coal on Tyne six feet thick. From thence to the Brockwell seam, which is little known on Tyne, is totally unknown on Wear, and is the lowest seam discovered either on the river Wear or Tyne." In a section so extensive as this taken by Mr. Forster, over a country broken and disturbed by numerous faults, it is possible that there may be some uncertainty with respect to the identity of the strata in particular situations; but the section may be regarded as a valuable approximation to truth.

average

average thickness is about nine yards. Below this limestone the great mass of whinstone in some parts makes its appearance: the thickness is from sixteen to sixty yards.

The lowest great limestone called the Melmerby scar limestone is by far the most considerable, being 42 yards in thickness. It is so called from its occurrence at Melmerby scar (or cliff) in Cumberland. This bed nearly equals in thickness the vast beds of limestone in Derbyshire, and Craven on the north-west of Yorkshire. Some few beds of limestone, but of minor importance, lie below the Melmerby scar limestone. The total thickness of the strata between the latter and the red sandstone is about one hundred and twenty yards. The red sandstone called by the Germans the old red sandstone, or the first flœtz sandstone of Werner, I believe, has no where been directly sunk through, nor is its exact thickness known.

Mr. Forster's section comprises, as before stated, both the coal and the metalliferous limestone districts, closing with the red sandstone. I have extended the horizontal sketch beyond the schistose and porphyritic mountains westward, where we again meet with thick beds of stratified sandstone and coal dipping into the Irish sea.

It is foreign to the purpose of the present letter to enlarge the account of this side of the island. I shall only observe that the rocks which compose the mountains near the lakes are well known to geologists to belong to that class which present few regular features of stratification (and I might add, with few exceptions,) nor any regular order of succession. The beds and tabular masses where these mountains are schistose are very elevated, or nearly vertical. Mr. Forster, whose observations appear to have been principally confined to stratified parallel rocks intersected occasionally by perpendicular veins and dykes, describes the whole mass of the Cumberland mountains cutting through the stratified rocks, as an enormous dyke or vein of what he calls blue rock. "The most remarkable dyke (he says) that we find in the north of England, is the great blue rock at Keswick, which in some places is ten or twelve miles wide, and may be traced into Wales to the southward and into Scotland northward." Extravagant as this description may at first appear, it coincides in substance with the system of those geologists who suppose that primary and transition mountains have been melted or softened by subterranean fire, and thrust through the superficial covering of the globe. I should not have thought it necessary to quote a description involved in the language of hypothesis; but I consider the opinions of practical men on any department of nature with which they are familiar, as deserving some attention, whether such opinions coincide with or oppose the fashionable theories of *cabinet* philosophers. The

The metallic veins which intersect the lower beds of this section are always most productive, as they pass through the limestone, and of the numerous beds of limestone that called the Great limestone is by far the richest in ore. The general direction of the principal veins is from east to west; but there are other veins which run from north to south, and intersect the above nearly at right angles. These are called cross courses. The first are called *right running* veins. The cross courses sometimes produce ore at their junction with the north and south veins, and at a little distance on each side of the latter. A curious fact is stated by Mr. Forster, that most of the east and west veins on Wear dale throw the strata on the north side or cheek of the vein up, and the vein declines to the south; but most of the veins at Alston Moor and Allen Dale throw the strata on the south side or cheek up, and the vein declines to the north. The beds of rock in this mining field being composed of different strata, calcareous, siliceous, and argillaceous; it is found that the nature of the rock affects the contents of the vein. In the calcareous the ore is most abundant; in the siliceous or sandstone beds the veins are seldom very productive of ore, and in the argillaceous strata, or plate, ore is scarcely ever found in the vein. It is also to be noticed, that the vein in passing the different beds becomes much narrower in the hard siliceous strata, and is sometimes nearly closed as it passes through them, but it again becomes wider in the limestone and plate. The inclination (or what is called the hade) of the vein is the greatest in passing through the soft strata, but in the hard strata above or below it takes its former inclination again, so as frequently to have a kind of zigzag course as it descends. It is also particularly deserving attention, that when the beds on one side of a vein are thrown up, and rocks of a different kind face each other, so that one side of the vein will have a wall of limestone and the other of sandstone or plate, the vein is seldom, if ever, so rich in ore as when both walls are of the same kind of rock; on which account it is found that the most productive veins are those where the rock on either side has but a small throw up, and of course the walls or cheeks of the veins (as they are called) are formed of the same stratum on both sides.

The almost invariable change in the quality or in the contents of metallic veins, as they pass through different rocks, is, I conceive, a demonstrative proof of the fallacy of Werner's theory, which represents veins as open fissures filled with metallic solutions from above, either by an aperture at the top or through openings in the sides. Were veins filled in this manner, the quality of the rock could have little influence on the ore. Werner quotes an instance at Kingsbergh in Norway, as if it were unique,

of

of the contents of the vein being richer as it passes some of the beds than in others. It would not have suited his theory to have admitted more; but the fact is, that so far from this being a rare occurrence, it is almost a general law, at least in England, where, I believe, mining operations have been carried on to a far greater extent and with more capital and skill than in any part of Germany. The principal substances which fill the veins in this mining district, besides galena or the sulphuret of lead, and blende or the sulphuret of zinc, are quartz, fluor spar, calcareous spar, iron pyrites, and pearl spar, a soft unctuous iron ochre which stains the fingers, and sulphate of barytes. Carbonate of barytes and carbonate of lead more rarely occur; copper pyrites is found in some of the veins, but not in sufficient quantity to be worked for that metal. Emerald green crystals of fluor spar occur in some of these mines, more beautiful perhaps than any that have elsewhere been met with. The carbonate of barytes is principally found in large detached balls, which have a radiated diverging structure. I have seen some of them not less than ten inches in diameter.

Mr. Forster relates a striking change in the barytic spar as it passes through different strata at Welhope in Northumberland. The vein in the sandstone strata contains sulphate of barytes (caulk); but when it enters the great limestone, the carbonate of barytes becomes the matrix.

The cubic crystals of fluor are sometimes very large, and often beautifully coated with minute brilliant rock crystals and with pearl spar. The phænomenon of quartz formed on cubes of fluor which have subsequently disappeared, is frequent in these mines, and has been long well known to English mineralogists. A similar phænomenon is observed in the mines of Derbyshire, where calamine is found formed on the metastatic crystals of carbonate of lime, commonly called the dog tooth spar; the crystals have been removed by some unknown process of nature. A similar effect also sometimes takes place with cubic galena, and the miners consider these operations as constantly going on; to use their expression, the "calamine is eating up the lead."

The mineral repositories in this district are the Rake vein; the Flat vein, which is a lateral expansion from the Rake vein; the Pipe vein, which is a flat vein compressed on its sides so as to form a tubular cavity; and the Accumulated vein, in which a number of veins converge, and unite, forming a kind of cone, filled with ore and veinstone. A more particular description of these will be given elsewhere.

The whole district is intersected by numerous faults, or dykes, which cut through and disturb the strata. *Dyke* in the provincial language of North Britain signifies a *wall;* and these

these perpendicular fissures or rents being generally filled with mineral matter harder than the rocks, it remains when the surface is in part worn away, rising up like a wall. Some of the dykes are of great extent, filled with whin-stone or basalt: but there is one which extends from the sea, running in a south-west direction, and has thrown down the strata on one side 180 yards: this is more properly a fracture of the strata than a dyke, the fissure being narrow and filled with clay. This fault passes near the town of Newcastle. It may deserve notice, that little if any appearance of the dislocation of the strata presents itself on the surface. Mr. Farey was, I believe, the first who particularly called the attention of geologists to the fact of the frequent disappearance of a whole series of strata on one side of a fault; in this instance it is truly remarkable.

There is a very powerful whin-dyke extending across the county of Northumberland from the sea in a direction westward: it rises to the surface, passing about four miles north of Newcastle, where it is extensively quarried, as well as in many other parts of its course. It is to be regretted that the information Dr. Thomson received respecting it was so erroneous, as it appears to have prevented him from examining it at Coaly Hill, where he might have seen its effects on the coal strata as it passes through them. Coaly Hill is not more than about four miles from the town of Newcastle: it is not a hill of basalt, but is composed of the regular strata which accompany coal. The whin-dyke is worked along a line of several hundred yards in length for materials to lay on the roads. The width of this dyke I do not accurately recollect; but there is a cart road along it where it has been worked, and a considerable space left between the road and the natural walls on each side. I am informed it becomes wider in the western part of its course. As it intersects the strata perpendicularly, and is worked to the depth of forty or fifty feet; the excavation forms a deep trench, on the sides of which the strata of sandstone and coal may be seen. I examined the coal which had been in contact with the basalt, it was charred, or reduced to a black powder. Wishing to obtain some information, from the man who was working in the dyke, respecting the general effect produced by the contact of the basalt with the coal, I requested my friend the Rev. W. Turner, of Newcastle, who was with me, to make the inquiry in the common language of the miners, and in such a manner as might not lead him to anticipate what were my own opinions. To the question: What has the whin-dyke done to the coal? The answer immediately was, 'She (meaning the dyke) has burnt the coal wherever she has touched it.' This man had worked at the quarries in the same dyke many years, and had frequent

frequent opportunities of seeing the whin-stone in contact with coal. Similar effects, I am informed, may be noticed in many if not in all the coal mines of this district, where the whin-stone immediately cuts through the coal. The whin-stone is intersected by fissures, and divided into blocks and balls: in some parts it graduates from basalt into an iron clay, which in one situation I saw formed into groups of minute regular pentagonal prisms: some of these I brought away with me. The whinstone of this dyke contains numerous small cavities filled with olivine; but in no specimens that I have seen from the dyke at Coaly Hill are there any cavities filled with chalcedony. I am therefore inclined to believe that the basalt laid on the road which Dr. Thomson saw, was not from this place, as a great variety of basalts and other stones are constantly brought from a distance as ballast to Newcastle, and employed for the roads. In some of these I have seen both chalcedony and zeolites.

There is a dyke filled with loose sandstone or rubble, which crosses a considerable part of the coal field without any disturbance of the strata. This, I believe, has been a mere fissure filled from above by materials washed into it. A similar dyke or fissure occurs in the Bradford coal field near Manchester, of which I have given an account in the second volume of the Transactions of the Geological Society.

Though I have spoken of the east and west basalt dykes as extending from the sea westward, I believe it would be more correct to describe them as streams of basalt diverging from a central focus in the mountains of Cumberland and Westmoreland beyond the mountain called Cross Fell. The summits of some of the mountains near Ulswater, particularly Swarthfell, are covered with basalt of a similar kind. On the western side of Northumberland the basalt rises in vast masses to the surface, and assumes the columnar form. In a direction towards the same focus may be traced the most considerable basalt dyke at present known in England, extending through the county of Durham and the north riding of Yorkshire to the sea between Whitby and Scarborough, of which a further account will be given in the second edition of my Introduction to Geology, now in the press. It is the same dyke which passes through the coal mines at Cockfield, and has produced the remarkable effect of charring the coal, and coating the under surface of the stratum above the coal with crystals of sulphur described in Mr. Bailey's Survey of Durham. The apparent agency of fire upon the coal extends to fifty yards on each side of the dyke. The Burtreeford dyke runs nearly north and south. These dykes, and the remarkable peculiarities of the metallic veins on the western side of Northumberland and Durham, offer a rich field to the labours of future geologists, but

but a transient or occasional visit will not suffice. A long residence in the district, and the free communication of numerous intelligent observers, are necessary to a complete investigation; on which account, the establishment of a Geological Society in that part of England would be particularly desirable. It is to be regretted that many gentlemen fully competent to observe, and to describe their observations, have been deterred from doing it by the appearance of the Wernerian Geognosy. The introduction of a new language, and of unfounded or undiscoverable distinctive characters, together with the loftiness of tone and manner in which these novelties were ushered to the world, terrified men of plain understandings from continuing or from publishing their own observations. Endeavouring, but in vain, to find in nature that regular conformity to the system of Werner, or, to use the words of an acute geologist*, distressing themselves "because they could not see what was invisible," they abandoned their labours in despair.

It remains to describe more fully the sketch, fig. 1, which I have drawn partly to explain the manner in which Mr. Forster's section was taken, by measuring the sinkings in different mines; and also to show the general arrangement of the rocks and strata beyond Mr. Forster's section. In a sketch of this kind extending more than one hundred miles, it was necessary to give the elevation of the mountains on an enlarged scale. Had the same horizontal and perpendicular proportions been retained, the highest point could not be more than the 1-20th of an inch above the line LL, supposed to represent the level of the sea. Notwithstanding the disparity on the scales of proportion, I trust the present sketch will convey a tolerably correct idea of the physical structure of this part of England, which is all I propose. I may be allowed to add, that I hope it will be received with some indulgence, being the first attempt that I am acquainted with to give a geological sketch of the succession of rocks across the island from the Irish channel to the German ocean.

The magnesian limestone A, which covers the east side of Durham, does not extend far into Northumberland, but can be traced in different situations along the coast; it has probably been washed away, as there are existing records of the great encroachments of the sea. By proceeding from B westward, the different strata rise to the surface till we come to the limestone district C C. The strata still continue to rise in the same direction, but are occasionally broken by dykes, one of which (the Burtreeford dyke X) has thrown down the strata on the west 160 yards. The red sandstone D makes its appearance on descending the

* Dr. MacCulloch.

deep

deep western declivities of the mountain limestone. We cross it in proceeding to Penrith, a few miles beyond which we meet with the mountains of slate, porphyry, gray wacke, hornstone, &c. marked E, that surround Ullswater and Keswick. The highest of these mountains, Sea-fell, is 3166 feet above the level of the sea. The summit is hornstone schistus. Further west we meet with siliceous sandstone marked F; and lastly, the Cumberland coal strata bordering the Irish channel G.

The limits of the present letter will not allow me to enter into the subject of the explosions in coal mines: but I think it proper to add, that I perfectly agree with what Dr. Thomson has said respecting the management of these mines, and the too great apathy shown by many of the proprietors to the waste of human life which annually takes place. I was informed that in two years six hundred persons were destroyed in the different coal mines of Tyne and Wear. By one explosion ninety-two persons were killed in the mines of Mr. Brandling in 1812; and the very week before the first meeting of the Society for preventing Accidents in Mines, twenty-three men and boys lost their lives in a pit belonging to Sir Ralph Milbank. At the request of the latter, and of the Rev. Dr. Grey of Sunderland, I attended that meeting. But I saw with regret, that the good intentions and exertions of the gentlemen most active in its establishment were viewed with jealousy by some of the coal agents and proprietors, as interfering with the rights of private property and tending to alarm the workmen. By the request of one proprietor the number of lives destroyed was erased from the resolutions, for fear of giving offence. I took the liberty of observing "that if all had been done which circumstances permitted, to prevent these fatal accidents, no one could be offended by a plain statement of facts: and in order to meet the difficulty fairly, and interest the public in promoting the object of the Society, it was necessary that the nature and extent of the evil should be fully made known." To render the mines secure would be attended with additional expense and labour; but the evil is of such magnitude, that, if a remedy be not applied, it will claim the serious attention of the Legislature.

I am, sir,

Yours, &c.

13, Tavistock-street, Bedford-square, Jan. 27, 1815. ROBERT BAKEWELL.

XVIII. *Ob-*

XVIII. *Observations on Mr.* DONOVAN'S *Reflections on the Inadequacy of the principal Hypotheses to account for the Phænomena of Electricity.* *By* G. A. DELUC, *Esq. F.R.S.&c.*

To Mr. Tilloch.

SIR,—1. THIS paper of Mr. Donovan will, I hope, be very useful in settling the doctrine of electricity, against which he finds the objections detailed in his paper, upon its true foundation.

2. The doctrine of *positive* and *negative* electricities, first published by Franklin, is true in itself; but by the manner in which he had expressed it it was involved in many difficulties which he had not foreseen. I shall have occasion hereafter to relate the opportunity which I had, long after, to demonstrate to him personally, by an experiment, what kind of influence the *air* has in the propagation of the electric fluid.

3. Franklin's theory was first attacked by Dr. Peart, who pointed out some phænomena of *electric motions* absolutely inconsistent with his statement of his own system. I shall not enter into that discussion, but point out directly the source of an insurmountable difficulty in that theory, as it was first expressed by its author, which circumstance (as I shall successively prove) has been the only cause of its rejection by Mr. Donovan. The error was this: Dr. Franklin considered as the *standard* of *plus* and *minus*, or middle point between them, a certain *natural quantity* of *electric matter* belonging to all the bodies of the earth: he called *negative* the bodies from which some part of that *quantity* was *abstracted*, and *positive* those to which a new quantity was *added*.

4. This error of Franklin has created all the just objections of Mr. Donovan; but from this circumstance I may judge that the latter has not had the opportunity of knowing two works which I have published, one in England in 1787, in two volumes, under the title of *Idées sur la Météorologie:* the other was published at Paris in 1804, also in two volumes, under the title of *Traité élémentaire sur le Fluide électro-galvanique.* In both these works I have applied Volta's system to the motions of a *pair of balls*, which motions are the most immediate test of electrical theories.

5. The essential and characteristic difference of Volta's theory, compared to that of Franklin, consists in the *standard* between *plus* and *minus.* Volta has demonstrated that there is no other *standard,* or middle point between these two opposite *electric states,* than the *actual* electric state of the *air,* which possesses

 electric

electric matter as well as the bodies which it embraces. But I must first relate the opportunity which I have had to learn that system from its author himself. Being at Paris in the year 1782, I made that very interesting acquaintance, and M. Volta was so good as to explain to me completely his system. The same year he came to London, and directed me in the construction of a more extensive set of electrical instruments, with which I made all the experiments related in the above mentioned works.

6. The principle of Volta with respect to *electric motions*, being that they have for their true *standard* the *actual electric state* of the *ambient air*, it came into my mind to submit that principle, as the foundation of the whole theory, to an *experimentum crucis.*

7. This experiment is related from p. 55 to 57 of the first volume of the *Traité élémentaire sur le Fluide électrico-galvanique;* where I first mention, as an indispensable condition for the success of that experiment, that the air be very *dry;* and I had the means, by my hygrometer, to determine the necessary degree of dryness, which is about 43° of my scale. As these experiments require some time, a greater quantity of *aqueous vapour* mixed with the *air*, dissipates too fast the *electric fluid* accumulated upon bodies, and transmits too fast some *electric fluid* to those which have been rendered *negative.*

8. I made this series of experiments in two contiguous rooms, separated by a short passage and a door. In one of the rooms I placed a very strong electric machine, by which the air could be modified without affecting that of the other room. I had a pair of *pith balls* with long conducting threads, suspended to a brass cap at the extremity of a *varnished glass rod,* and thus thoroughly *insulated,* as well as the ferrule to which the *balls* were suspended. Before the electric machine was put in motion in its room, the *balls* had no *divergence* in either of them.

9. In one of the experiments, I fixed a point at the extremity of the *prime conductor* of the machine, the *rubber* being placed in communication with the ground: a *luminous brush* appeared at the *point,* indicating that the *electric fluid* escaped from it and was communicated to the *air* of the room. Now, when I brought the *pair of balls* from the next room, where they did not *diverge,* they strongly diverged in the room of the *machine;* and by the test of a rubbed stick of sealing-wax they were found to diverge *negatively,* because the *air* had acquired some *electric fluid* by the action of the *positive point,* and thus was made *positive;* but when the *balls* were brought back to the other room, they ceased to *diverge.*

10. I made the inverse experiment, by fixing a *point* to the *rubber* of the machine, its *prime conductor* being placed in communication

munication with the ground. The machine being worked in this state, a *luminous point* was seen at the extremity of the point fixed to the rubber, indicating that some *electric fluid* passed from the *air* of the room to the *rubber*, and thus that *air* was made *negative*. Now the *pair of balls*, which did not again *diverge* in the next room, being brought into that wherein the machine was, they strongly *diverged*, and by the test they diverged *positively*, being brought into an atmosphere rendered *negative* by the *point* fixed to the *rubber*.

11. These experiments cannot leave any doubt, on Volta's theory, that in the divergence of a pair of balls, the *standard* of *plus* and *minus* is not, as Franklin had determined it, a *natural electric state* of bodies; but that it is the *actual* and *variable* electric state of the *ambient air*. Had Mr. Donovan been acquainted with that theory of Volta, still retaining the fundamental theory of *positive* and *negative*, first announced by Franklin, but changing the standard from a *fixed* to a *changeable* state well defined, he would certainly have retained the fundamental doctrine of *positive* and *negative* with that correction.

12. As to the theory of Æpinus, that *electric atmospheres do not exist*, which Mr. Donovan seems to prefer to that of Franklin corrected by Volta,—I have shown in one of my works, that Æpinus, instead of simplifying the theory, has fallen into a greater complication of hypotheses, even contrary to some general laws of nature. But I shall not enter into that discussion, as Volta's system renders it unnecessary.

13. It is by this influence of the *air*, receiving its share of the *electric fluid* possessed by the bodies which it surrounds, that Volta explained the motions of electrified balls, and the following is his explanation. "When two balls suspended near each other are in a *positive* state comparatively to the *air* of the place, they *both* communicate some *electric fluid* to the *air between them*; while each of them communicates *alone* the *electric fluid* to the *outward air*. Each of the balls therefore moves towards that *outward air* on each side, not by *repelling* each other, but by moving towards the *air* possessing *less* electric fluid.

14. During the course of my experiments to demonstrate, in various manners, the certainty of Volta's system on the cause of motion of electrified balls, it came into my mind that an analogous phænomenon might be produced, in which the *cause* of the motion might be *visible*; and it having succeeded, I have described this experiment from p. 116 to 119 of the same volume, of which it will be sufficient to give here an extract.

15. An accidental observation led me to that experiment, which those who shave themselves and employ *soap powder* will easily repeat. If the basin used happens to be covered with

dust, this swims over the water; but it is very easily swept, by scraping a small flake of soap, taking it on the point of a knife, and endeavouring to let it fall on the middle of the surface of the water with its convexity undermost. As soon as that flake of soap swims on the water, the *dust* recedes from it, and ascends to the edge of the water on the side of the basin, where it forms a thin fringe.

16. Having taken notice of that motion of the surface of water made visible by the *dust*, I attempted to imitate the motion of *two electrified balls* by two *disks of soap* half an inch in diameter, and about 1-10th of an inch thick, in the centre of which I fixed with gum a very thin thread four inches long, and I suspended these disks to a slip of wood at such a distance that they slightly touched each other. When that little apparatus was prepared, and the two disks made to rest at the same time on the surface of the water, they began to recede from each other, and the motion of the dust on the surface of the water visibly demonstrated that the *disks of soap* moved both towards the water which had not yet dissolved *soap*.

17. Lastly, I have demonstrated by a direct experiment, which I shall relate hereafter, that a perfect *vacuum* does not transmit the *electric fluid;* which fact will be a peremptory proof of the essential interference of *air* in the motion of a pair of *electrified balls*.

18. Another essential part of Volta's system, with which Mr. Donovan is not acquainted, and which when fully understood removes all the difficulties he has found, is that which Volta has called *electric influences*. It consists in this effect: when a body *positive* is brought near one of the extremities of an *insulated conductor*, it gives more *tension* to the *electric fluid* at that end of the *conductor*, and makes it recede to the further end. This principle removes all the difficulties that Mr. Donovan has found, and in particular that which seems to arise from his experiment related in p. 338, of a pith-ball suspended to a glass rod, and an excited glass tube brought under it. The excited tube occasions a slow retreat of *electric matter* along the thread. While that matter remains together in the ball, the thread, and at the point of suspension, the ball is repelled by the excited tube; but at last the suspended ball loses some electric matter, and being then *negative*, it is attracted by the excited tube.

19. The author attacks with reason the cause assigned by Franklin to the *luminous brush* which appears at the extremity of a pointed body fixed to the prime conductor of an electric machine in motion. But Volta has also explained this phænomenon in a satisfactory manner, as I have explained in pp. 60 and 61. Suppose a conductor *positively* electrified, and to which is presented another conductor in communication with the ground, but

but the surface of which is of a certain extent; the particles of *air* in motion, returning from the electrified conductor, and thus possessing more electric fluid, might transmit some to the other body; but their *influence* on it producing a diminution of *tension* in the fluid proceeding from the *conductor*, at the same time that they increase the *tension* in the fluid of the body, there is thus a very small disposition of the *electric fluid* to abandon the *particles of air*. But if it be a *point*, the *electric fluid* belonging to it is in too small a quantity to increase the *tension* of that belonging to the body: thus, every particle of *air* which comes in contact with that body discharges its electric fluid, returns instantly, and, as it takes the shorter way, thus is produced a *current of air*. The same alternate motions take place in the particles of air, whether the point is *negative* or *positive*.

20. The difficulty concerning the *impermeability* of *glass* to the *electric fluid*, which the author opposes to the phænomena of the Leyden vial, as accounted for by Franklin, has also been removed by Volta, by supposing the *electric fluid* composed of two ingredients, to one of which only *glass* is *impermeable*, but the other passes readily through it. If the jar is coated on both sides with tin-foil up to a certain height, it will not receive a *charge*, unless the outside coating is in communication with the ground.

21. On the explanation of this common phænomenon rests the whole theory of *positive* and *negative* electricities, which Volta, having placed on its true foundation, has thereby clearly explained the phænomena of the *Leyden vial*; and it is his theory which has led me to my systems on the nature of the *electric fluid*. This fluid, considered in its common phænomena, is composed of two ingredients; one, which does not possess expansibility by itself; the other, which, united with it, gives expansibility to the compound. Of this union I have given an example in the *aqueous vapour*, which is composed of particles of *water*, a substance not possessing expansibility, and of particles of *fire*, which gives expansibility to the compound.—That analogy, with all its particulars, I have explained in p. 77, section I. with this title: "Analogies and Differences of the Electric Fluid, and the Aqueous Vapour." In consequence of this analogy, I have given the name of *electric matter* to that ingredient of the *electric fluid* to which *glass* is not permeable, and *deferent fluid* to that ingredient which *transports* the *electric matter*, and which alone passes through the glass.

22. In this system Mr. Donovan may find the solution of his difficulties concerning the charge and discharge of the *Leyden vial*. Franklin's system is true so far as he supposes that the

vial possesses (nearly) the same quantity of *electric fluid*, both when it is in its *natural state*, and when it is charged; but the *electric fluid* is not distributed in the same manner in both cases. When an *electric machine* tends to produce an accumulation of *electric fluid* on one of the surfaces of the *vial*, it cannot accumulate except the outside of the *vial* can part with an equal quantity, its coating being in communication with the ground. But it is not the identical *electric fluid* which pervades the glass, the *electric matter* thrown in remains fixed on the inner surface; the *deferent fluid* alone pervades the glass, and, uniting with the *electric matter* on the outward surface, forms there an equal quantity of *electric fluid*, which is transmitted to the ground if it finds a conductor. This is a complete analogy with one of the phænomena of the *aqueous vapour* mentioned in the above quoted *section*. The *maximum* of *charge* is attained, when the quantity of *electric matter* accumulated on the internal surface is in equilibrium with that transmitted by the electric machine.

23. So far this system might appear only an *hypothesis* to Mr. Donovan, as he does not know the work to which I refer, where I have given a peremptory demonstration of it, derived from the *electric motions*, proving by a direct experiment, that these motions refer only to the *electric matter*, without interference of the *deferent fluid*, except to transport the *electric matter*.

24. In tome ii. p. 8, I have related an experiment which very much struck Mr. Cavallo, in presence of whom I made it at Windsor. The apparatus for that experiment is described, with a plate, in my work; it consists of a conductor supported in an horizontal position on an insulating pillar. At one end of this conductor are suspended, by brass wire, two cork-balls half an inch diameter going down so as to hang opposite to the centre of two insulated brass disks two inches diameter, brought to about $3\frac{1}{2}$ inches of each other, and the balls hung at equal distance from them. I give a spark to each of the disks with a Leyden vial: the *deferent fluid* of the disks, thus rendered *positive*, and giving more expansive power to the fluid of the balls, a part of it is carried towards the other extremity of the insulated conductor; and the balls losing thus a part of their *electric matter*, they diverge as being in a *negative* state, and are carried on both sides towards the positive disks, which they would strike, were they not prevented by a kind of bridle, placed at the end of the conductor, showing by small motions as it were an avidity to move towards them.

25. In that state of the apparatus in which the *influence* of the disks, being *positive*, gives more expansive power to the *elec-*

tric

tric fluid of the *balls*, they are certainly *negative* comparatively to the *ground* and the *ambient air*; but the *deferent fluid* of the *positive disks* giving more *expansive* power to their *electric matter*, a *conductor* communicating with the ground may be brought into immediate contact with them without effect: their *negative divergence* still continues. The same phænomenon is produced by inversely electrifying the horizontal insulated conductor and the disks: when the balls, then *positive*, are made to communicate with the ground by a brass conductor, they continue to *diverge*, being under the *influence* of the *negative atmosphere* of the disks.

26. These phænomena much interested M. Cavallo, who was a very liberal-minded man. He persisted in his opinions as long as he found reasons to defend them, but was ready to abandon them whenever his arguments appeared to be contradicted by facts, to which he was very attentive. Having made some stay at Windsor, he came often to me and proposed some alterations in my apparatus, which he thought could change the phænomena. I foretold him what would happen by the changes, some of which would change the effect on account of causes which I pointed out, and others would produce the same effect. At last he saw and acknowledged that those phænomena could absolutely not be explained but by admitting the *composition* of the *electric fluid* of *two* ingredients, one of which only, which I had called *electric matter*, produced *electric motions*, and the other, which gave expansibility to that *matter* in proportion to its relative quantity, which I named *electric deferent*. Thus Volta's theory, applied to all the electric phænomena which were opposed to Franklin's theory, in his own expressions, have cleared the system of *positive* and *negative* electricities from the objections of Mr. Donovan, arising from the erroneous principle which he admitted in it, of a *natural quantity of electric matter* belonging to all terrestrial bodies.

27. I come to another very important object in the science of electricity, that of the effect of *friction*, which in vol. xxxiii. of Mr. Nicholson's Philosophical Journal I have explained in a manner different from that of Mr. Donovan in p. 337. His hypothesis is this: "That during attrition between bodies, one of which must be an *electric*, the pores of the latter being *open* will receive the *plus* quantity, and will give it out again when the *pores* close."—In this respect I have proved, that there is no other distinction between bodies than that of *conductors* and *non-conductors*; the latter of which only can be excited by *friction*, because the electric fluid, thus set in motion on the surface, moves slowly to make its escape by a *conductor* offered at a di-

 stance.

stance. While, if both bodies which exercise *friction* on each other are good conductors, the equilibrium is incessantly restored; but if one has more disposition than the other to attract the electric fluid thus agitated, with the faculty of transmitting it to its remote parts, when the bodies are separated before the *equilibrium* is restored between them, one is found *positive* and the other *negative*.

28. These effects are distinctly demonstrated by a small *electric machine*, the figure of which is at the head of my paper in Mr. Nicholson's Philosophical Journal for January 1811, under the title of "Experiments concerning the Electric Machine, showing the Effects of Friction between Bodies."

29. M. Cavallo has given a table containing the results of his experiments of this kind, wherein we find that certain bodies become either *positive* or *negative* by *friction*, according to those by which they are *rubbed*; but the manner in which his experiments were made did not indicate the effect produced on the *rubber* itself, because it was not *insulated*. I thought it therefore very important to the doctrine of electricity, to have both effects indicated by electrometers. This I obtained by the apparatus described in that paper; in which the bodies rubbed are *spindles* turned by a winch, and various *rubbers* made to press on the *spindles* by proper springs. A small insulated *prime conductor* is connected by one of its extremities to the *spindles*, and by the other with a *gold-leaf electrometer*. The *rubbers* are *insulated*, and each of them when applied is made to communicate with a similar *gold-leaf electrometer*.

30. The constant result of these experiments was, that the quantity of *plus* on one side was equal to the quantity of *minus* on the other. Now the *friction* being reciprocal, the supposition of the *opening of the pores* cannot explain that phænomenon, since they ought to be *opened* equally in the *rubbing* as in the *rubbed* bodies.

31. The effect of friction, as I have said above, is to set in motion on the surface of the bodies, and that, if the body which recedes from the point of friction finds in its way a pointed conductor, it transmits to the latter a part of that *fluid*, which is the effect of the common electric machine: but the same phænomenon extends further; for that effect takes place between bodies of the same kind, if they be *non-conductors*. This is proved by Exper. 3, p. 9, of the same number of the Philosophical Journal.

32. A flat piece of the same *glass* as the *spindle*, being held at the end of a brass spring, and used as a *rubber* on the *spindle*, holding the brass part in my hand near the *glass*, in order to restore

restore from the ground the electric fluid carried away to the prime conductor by the spindle, the *gold-leaf* of the electrometer diverges as *positive*, though the *rubber* is of the same substance as the *spindle*. This is a peremptory demonstration that the effect of *friction* is not to *open* the pores for receiving more electric matter, which is discharged when the *friction* ceases, as Mr. Donovan conceives it: since both bodies, which in this case exercise friction on each other, are of the same *glass*.

33. Exper. 4, p. 10, shows that *sealing-wax*, used as *rubber* over the *glass spindle*, becomes strongly *negative*, and renders the *glass* strongly *positive*. And thus it is directly proved, that the cause of *sealing-wax* being rendered *negative* by *friction* with the *glass*, is that the latter takes some *electric fluid* from *sealing-wax*, which effect could not have been ascertained without the two *electrometers*.

34. Now, in Exper. 1, a *brass rubber* applied to the *glass cylinder* is seen to become *negative*, and the *glass cylinder* made *positive*; but in Exper. 6, having covered a *glass cylinder* with a thick coating of *sealing-wax*, producing in fact a *sealing-wax cylinder*, the same *brass rubber* was rendered *positive*. These experiments compared with each other, further demonstrate the real *electric* effect of *friction*. But the next experiment will place it beyond all doubt.

35. Exper. 8 is made with what is called *India beads*, the size and colour of a cherry: they are made, as I have been informed, of an inspissated vegetable oil, very elastic, not soluble in water. I placed one on a glass spindle, and fitted to its shape two narrow *rubbers*, one of *naked brass*, the other of *brass* covered with *sealing-wax*. With a very small motion of the winch, that *bead* became *negative*, and the *naked brass rubber* became *positive*; but the same *bead* became *positive* when the *brass rubber* was covered with *sealing-wax* and this *rubber* was made *negative*.

36. These experiments afford a true analysis of the *electric* effects of friction. Its general effect, as I have stated, is to set in motion the *electric fluid* residing on the surface of the bodies which are rubbed; and the consequence of that effect is, that the body which recedes from the point of *friction* carries along with it a part of that *electric fluid*; which effect extends even to the case when the *rubber* and the body *rubbed* are of the same substance, when the latter suddenly recedes, and meet in its way a *conductor*, before returning to the *rubber*.

37. These experiments remove the difficulty which Mr. Donovan found (p. 339) to reconcile the (supposed) *equal distribution* of the electric fluid with the *impermeability* of glass. The *equal*

equal distribution is gratuitously supposed, since *glass* is but an imperfect *non-conductor* when not covered with some resinous varnish.

38. Mr. Donovan says (p. 345): "As to the fact whether *glass* is actually *impermeable*, many experiments have been made; but they all appear to be of doubtful force, and may be explained in some manner, without supposing that electricity passes through."

39. I have made in this respect some decisive experiments, proving that when a *glass rod* transmits the *electric fluid*, it is only by its surface. I used for that purpose three *glass rods* of the same glass and the same diameter—one remaining *naked*—another covered all over with *sealing-wax*—the third covered with *sealing-wax*, with only an interruption in the middle of its length. These rods were supported horizontally on insulating pillars, which left their extremities accessible to the knob of a Leyden vial. I used three very sensible *electrometers*, each having a long brass conductor in order to connect them with the rods.

Exp. 1. Having placed the *naked rod* on the pillars, with an electrometer at each end and one in the middle, and applied the Leyden vial to one extremity, the electrometer near it first diverged, then that in the middle, and soon that of the other extremity, showing that some time was required for the propagation of the electric fluid even on the naked surface of *glass*.

Exp. 2. Having used the rod entirely covered with *sealing-wax*, a small motion was produced in the next electrometer, by the influence of the vial, but none in the remote electrometers.

Exp. 3. The rod with an interruption of the *sealing-wax* in the middle of its length was to be the test. If *glass* were *permeable*, the electrometer applied to that part ought to have been put in motion; but if it moves only along the surface of the *glass*, being stopped by the *sealing-wax*, the electrometer applied to the naked part in the middle cannot be put in motion. The result of the experiment was, that, whatever time the Leyden vial remained applied to that extremity, no motion was produced in that electrometer, any more than in that of the other extremity. It is therefore demonstrated that *glass* is absolutely *impermeable* to the *electric fluid*.

40. A very specious objection of Mr. Donovan against Franklin's doctrine is thus expressed, p. 349: "Franklin supposes that no *electricity* can be received on one surface, unless the other can part with an equal quantity. In the case of excitation of the *common cylinder*, the *inner surface* having no communication

cation with *conductors*, can part with none. How then can its outer *surface* receive the great quantity we find in it?" The answer is however obvious, for this is the common effect of *friction*. The cylinder receives constantly *electric fluid* from the *rubber*, especially when this is covered with a *metallic amalgama*; for it is in communication with the ground. In that case it is not necessary that the opposite surface of the glass should part with any *electric fluid*; the whole process takes place on the *outward surface*. The *rubber* gives *electric fluid* to the *glass cylinder*, which parts constantly with it, in meeting the prime conductor; but the *rubber* communicating with the ground, furnishes also constantly a new quantity of the fluid.

41. It remains only to state a very essential point in electric phænomena, namely, How does the electric fluid communicate itself through space? This was the object of an experiment which I made in 1774 in Mr. Walsh's laboratory, which experiment in the first volume of my work, *Idées sur la Météorologie*, p. 521, I left under Mr. Walsh's name, because he had published it, without my knowledge, in the Philosophical Transactions for 1785. But these experiments were concerted between Dr. Franklin and myself, and only in the house and in presence of Mr. Walsh, with an apparatus of which the description will show the purpose and its origin.

42. In my experiments for producing a truly comparable *barometer*, repeating those of a French academician, Du Fay, for producing *light* in the *vacuum* of barometers, an experiment related in the *Mém. de l'Académie des Sciences de Paris* for 1723, I found, as has been explained in my work, *Recherches sur les Modifications de l'Atmosphère*, tom. i. p. 43, that when the *Torricellian vacuum* was procured by making the mercury *boil* in the tube, no light appeared at the top of the barometer; whence I concluded, that a perfect *vacuum* was not a *conductor* of the *electric fluid*.

43. Having expressed this idea to Dr. Franklin, he proposed to me an experiment, very difficult to execute, but which he encouraged me to undertake. The apparatus consisted of a glass syphon, the legs of which were about three feet distant from each other; the curve began about three inches above the point at which stood the common barometer at that moment. When the syphon was filled with mercury it was inverted, with its two legs plunged into separate cups, each resting on an insulating stand: these cups received the mercury descending from the upper curve, and the column thus separating, there were two barometers with a common vaccum.

44. In that situation, when a spark was given with a Leyden vial

vial to one of the cups, a luminous arch was seen filling the top of the syphon, and a spark could be drawn from the other cup, thus showing that it was not a conductor of the electric fluid.

45. There remained to be performed the second part of the experiment and the most difficult, namely, to have the mercury *boil* in that long syphon. I succeeded in this operation; and when the syphon was placed with its legs in the cups, that *complete vacuum* ceased to be a conductor. The rain sparks were given to one cup, none was drawn from the other.

46. This experiment convinced Dr. Franklin of my system, that the *electric fluid* was a sort of *parasite* substance, which, on our globe and its atmosphere, was distributed on all other matters, and nowhere accumulated so as to produce *lightnings*, and their common attendants *thunders;* but that these phænomena proceed from the decomposition of the *atmospheric air* by a certain process, which manifests that the *electric fluid* enters into the composition of that *air*, a fluid *sui generis*, and not a mixture of different kinds of *air*, as it was supposed in the new chemical theory. Which conclusion of my long course of experiments serves as the basis of my work under the title of *Introduction à la Physique terrestre par les Fluides expansibles.*

XIX. *Queries, as to Grindstones and Ironstone in Durham, and Shells, &c. near Cambridge.*

To Mr. Tilloch.

Sir,—Having read in a cotemporary journal, that Nathaniel Wynch, Esq. has presented to the Geological Society an account of the Geology of Durham and Northumberland, and has mentioned therein, that the "grind-stone sill" furnishes the celebrated Newcastle Grindstones (dug on Gateshead Fell, as I am informed): now on reference to Mr. Westgarth Forster's "Treatise on a Section of the Strata," pages 149 and 31, I find him describing the Grindstone sill as nearly the uppermost on *Cross-fell* mountain, and as such, much lower in his Section than the Gateshead-fell Rock, I should suppose, but on which I beg to receive information through your work, from Mr. Wynch, Mr. Forster, or any other person, who can point out the particular stratum in Mr. Forster's Section, wherein the Gateshead Grindstone Quarries are situated.

Mr. Wynch has likewise mentioned, that "bivalve shells" occur in the clay Ironstone Balls lodged in one or more beds of shale or slaty clay: and I shall also be obliged by information, as above, whether the shells mentioned resemble fresh-water or

Pond

Pond *Muscles* but much smaller? and to which particular stratum or strata in Mr. Forster's Section, they belong.

In the same work I observe it mentioned, that Professor Hailstone, of Cambridge, has presented to the Geological Society an account of Fossils found near that place, partly in a bed of calcareous blue clay, called *Galt*, which he considers as the lowest bed of the Chalk formation, and which therefore should seem to be the *Chalk Marl* of several modern writers; whereas *Galt* is more commonly applied to *alluvial Clay*, in the district alluded to. I wish therefore to inquire of Mr. H. or any other of your readers, whether some of the extraneous fossils there mentioned, such as charred wood, mutilated Fish, Pentacrinites, &c. were not found in moved and water-worn *alluvial Clay*, instead of their being imbedded in stratified *Chalk Marl?*

I am, &c.

Feb. 2, 1815. A CONSTANT READER.

XX. *Letter from M.* AMPERE *to Count* BERTHOLLET, *on the Determination of the Proportions in which Bodies are combined, according to the respective Number and Arrangement of the Particles of which their integrant Molecules are composed.*

[Continued from p. 43.]

If we now proceed to consider the primitive forms of the crystals recognised by mineralogists, and regard them as the representative forms of the most simple particles, admitting into these particles as many molecules as the corresponding forms have summits, we shall find that they are five in number; the tetrahedron, the octahedron, the parallelopipedon, the hexahedral prism, and the rhomboidal dodecahedron.

The particles corresponding to these representative forms are composed of 4, 6, 8, 12 and 14 molecules; the three first of these numbers are those which we require to explain the formation of the gases to be immediately cited. I have shown in my memoir that the number 12 is that which we must employ in order to express the composition of the particles of several very remarkable combinations, and that number 14 accounts for that of the particles of the nitric acid, as it will be if we can obtain it without water, for that of the particles of the muriate of ammonia, &c.

Let us now see how the molecules may be united according to these different forms:

Two molecules being supposed to be united on a line, to give a clearer idea of their respective position; if we add thereto two

two other molecules united in the same manner, at first in one and the same plan, so that the two lines mutually cut each other into two equal parts; and if we afterwards remove them, by keeping them always in a situation parallel to that which they had on this plan, we shall obtain a tetrahedron, which will be regular in the case only where the two lines were equally perpendicular to each other, and where they have been removed from each other to a distance, which is to their length as $1 : \sqrt{2}$.

Let us now conceive three molecules joined by lines forming any given triangle; let us place in the same plan another triangle equal to the first, and of which the situation is such that the two triangles have their centre of gravity at the same point, and their equal sides respectively parallel. Separating these two triangles, so that the three sides of each triangle may remain constantly parallel to their primitive position, we shall obtain six points placed as they ought to be to represent the six summits of an octahedron, which will be regular only in the case where we have thus joined two equilateral triangles, and where we have separated them perpendicularly to their plan from a quantity which is to one of their sides as $\sqrt{2}$ $\sqrt{3}$.

If we suppose in the case of the tetrahedron which we draw by the two lines of which we have spoken, two plans parallel between them, and we place in each of them a line which represents the position in which will be found the line of the other plan before they had been separated, the extremities of these two new lines will be the four summits of a symmetrical tetrahedron at the first, which shall have its centre of gravity at the same point, and the eight summits of those two tetrahedrons joined in this manner will be those of a parallelopipedon. It is thus that the parallelopipedon form results from the union of two tetrahedrons. It is easy to see that when the two tetrahedrons are regular the parallelopipedon becomes a cube; a rhomboidal parallelopipedon when the tetrahedrons are regular pyramids; a straight prism with rhomboidal bases when four ridges of every tetrahedron are equal to each other; and finally, the base of this prism becomes a square when to this condition is added the equality of the two other ridges. In the case of the octahedron, if we place in the same way in the plans of two triangles, of which we have spoken, those which represent the position in which will be found the triangle of the other plan before they had been separated, the six angles of these two new triangles will be the six summits of an octahedron symmetrical to the first, which shall have its centre of gravity at the same points; and the twelve summits of those two octahedrons, thus joined, will be those of a hexahedral prism: this form results, therefore,

fore, from the junction of two octahedrons. The hexahedral prism will not be straight, unless so far as we shall have removed the two first triangles in a direction perpendicular to their plan, and it will have for its base a regular hexagon only in the case where those two triangles are equilateral. We may remark, that in the hexahedral prism formed in this way with two regular octahedrons, the height is to the sides of the bases as $\sqrt{2} : 1$.

In general, the examination of the circumstances which result from the regularity or irregularity of the particles which are united to each other, as two tetrahedrons are in order to produce a parallelopipedon, and two octahedrons in order to give birth to a hexahedral prism, requires very complex considerations, which are of no use to the explanation of the theory which I am describing, while we are merely occupied with the number of the molecules of each particle, and cannot have an application. But when we study under this point of view the primitive forms of the crystals given by observation, I shall put them out of consideration in this extract; and as it will only be necessary to speak of the number of the molecules of which the particles formed by the union of other particles already known are composed, I shall consider as regular all the tetrahedrons and octahedrons, the various combinations of which I shall examine. It will be easy, by the help of a few reflections, to form an idea of the modifications which the results of this examination will undergo in cases where these polyhedrons are irregular.

It is evident, that by placing at the same point the centres of gravity of two tetrahedrons and an octahedron, so as that the two former should make a cube, and the situation and dimensions of the octahedron are such that the ridges of this cube and those of the octahedron mutually cut each other at right angles into two equal parts, the polyhedron with 14 summits which will result from their junction will be the dodecahedron, the last of the primitive forms given by the mechanical division of the crystals; for we ought not to reckon among these forms the double pyramid with hexagonal bases, admitted at first in order to explain the crystallization of quartz, and brought back afterwards to a parallelopipedon.

It appears from what we have said, that when particles are united into a single particle, it is by placing themselves in such a way that, the centres of gravity of the component particles being at the same points, the summits of the one are placed in the intervals left by the summits of the others, and *vice versâ*. It is in this way that I consider chemical combination; and here it differs from the aggregation of similar particles, which takes place by simple juxtaposition, as is seen in that elegant theory of crystallization which the sciences owe to M. Haüy. It is also

also in this way that I have obtained, by combining other numbers, tetrahedrons and octahedrons, the various representative forms required for the explanation upon the same principles of all the combinations in a determinate ratio which are known to me.

On attempting to join tetrahedrons and octahedrons in all possible ways, we find that there result from most of them representative forms in which the various molecules are arranged in an irregular manner, and that there are some in one direction, without there being any in another direction corresponding to the first. All these forms ought to be rejected, and we observe in fact, that the proportions which they suppose in chemical combinations are not met with in nature. If we try, for instance, to combine tetrahedrons and octahedrons, so as that the number of the former shall be the half of that of the latter, we find only awkward forms which do not present any regularity, or any proportion between the relative sizes of their different faces. Hence we ought to conclude that a body A, the particles of which have for their representative form tetrahedrons, and a body B, of which the particles are represented by octahedrons, will not unite so as that there shall be in the combination one proportion of A and two proportions of B: on the contrary, this combination will be easy between two proportions of A and one of B, since two tetrahedrons and one octahedron form by their junction a dodecahedron. In the same case, A and B will unite in equal proportions by means of two forms which I shall describe, and in which the number of the tetrahedrons is equal to that of the octahedrons.

I. An octahedron may be joined with a tetrahedron, by placing the summits of the octahedron on the prolongations of the lines which, issuing from the centre of gravity of the tetrahedron, pass by the middles of its six ridges: we thus form a polyhedron with ten summits and sixteen triangular faces, four equilateral and twelve isosceles, to which I shall give the name of hexadecahedron.

2. Two octahedrons joined in a hexahedral prism may be joined with two tetrahedrons forming a cube, in a manner analogous to that in which an octahedron is united to a cube in the dodecahedron. In order to form a clear idea of this combination, we must consider one of the diagonals of the cube as the axis of this polyhedron, and elevate for it a plan perpendicular passing by the centre of the cube. This plan will cut six of its ridges into two equal parts, the points of division being situated like the six angles of a regular hexagon, by placing thereon the middles of the six vertical ridges of a hexahedral prism formed by the junction of two regular octahedrons: the twenty

twenty summits of this polyhedron will be those of a new polyhedron which will have 30 faces; viz. six rectangular parallelograms and 24 isosceles triangles: I shall give it the name of triacontahedron.

It is easy to see from this construction that the diagonal of the cube is equal to that of the prism, and that in this way all the summits of the triacontahedron are in one and the same spherical surface.

It will be in vain to endeavour to form other combinations presenting some regularity by combining two of the foregoing polyhedrons. Let us proceed to another mode of combination. If we consider twelve points placed with regard to each other as the middles of the twelve ridges of a cube, these points will be situated by fours in three rectangular plans: hence it follows, that if we place at the first four the four angles of the square base common to the two pyramids of which one of the octohedrons is composed, to the other four the four angles of the base of a second octohedron, and to the other four those of a third octohedron, the summits of the three octohedrons will be two and two in the intersections of the three rectangular plans, and these 18 summits will be those of a polyhedron with 32 triangular faces, eight of which will be equilateral and 24 isosceles: I shall give to this polyhedron the name of trioctohedron, which refers to its generation.

The trioctohedron may, like the octohedron, be combined with two tetrahedrons forming a cube: for this purpose we shall prolong the plans of its triangular isosceles faces from the side at which they are joined with the equilateral faces until these plans meet by threes outside of the polyhedron opposite those last faces. The eight points thus determined are evidently situated with respect to each other like the eight summits of a cube: hence it follows that we might thereon place the eight summits of two tetrahedrons, the union of which with the trioctohedron will form a polyhedron with 26 summits and 24 equal quadrilateral faces. The trapezoidal form of the mineralogists is a particular case of this form, which results from a certain proportion between the axis and the sides of the square bases of the straight octohedrons, of which we may conceive the trioctohedron to be formed. I shall in general preserve the name of trapezoidal, as expressing a property always belonging to it, whatever are the dimensions of these octohedrons.

It is not with tetrahedrons as with octohedrons: we cannot unite three of them in a polyhedron which presents some regularity; but there exists one formed by the combination of four tetrahedrons. In order to obtain it, we shall consider four points

situated

situated as the four summits of a tetrahedron equal to the four which we wish to join, and we shall conceive that at each of these points is placed one of the summits of each tetrahedron; whereas the three other summits of the same tetrahedron are in the plan which passes by the three other points, and correspond to the middles of the intervals which they leave between them. I shall give to the polyhedron resulting from this combination of four tetrahedrons so united, the name of tetra-tetrahedron. This polyhedron has sixteen summits and twenty-eight triangular faces; four of which are equilateral and twenty-four isosceles.

We shall easily demonstrate, that if we prolong the plans of the twelve isosceles faces adjacent to the four equilateral faces of the side where they join to those faces, the prolongation of those plans will meet by threes outside of the tetrahedron, in four points corresponding to the middles of its four equilateral faces, and which will be the summits of a fifth tetrahedron equal to the four preceding ones: by uniting it with them, we have the twenty summits of the polyhedron which I have called penta-tetrahedron, and which has twenty-four faces; viz. twelve quadrilaterals and twelve isosceles triangles.

If we again consider twelve points situated with regard to each other as the middles of the twelve ridges of a cube, and place a tetrahedron so that, its centre of gravity being at the same point with that of the cube, two of its ridges opposite pass by four of these points; and if we do the same thing in succession with respect to five other tetrahedrons, in order that the number of the summits should be the same in all directions, we shall obtain a polyhedron with 24 summits and 14 faces, six squares, and eight hexagons, which I shall call hexa-tetrahedron.

These hexagons, all equal to each other, will have each three sides greater and three smaller, which will be to each other as $1 : \sqrt{2}-1$.

This polyhedron is evidently an octohedron only, of which the summits are truncated by plans perpendicular to its three axes: its combinations with other representative forms are more numerous than those of any of the preceding polyhedrons.

We may at first combine it with an octohedron situated in such a way that, having its centre of gravity at the same point, all the faces and all the ridges of this octohedron shall be parallel to those of the octohedron; from which we may conceive that the hexa-tetrahedron is the result of *truncatures*, by being solely subjected to the condition of its dimensions being less than those of the latter, in order that the polyhedron thus formed may

may not have re-entering angles. This polyhedron only differs from the hexa-tetrahedron in having, besides the latter, regular pyramids raised on its square faces. I shall call it a pyramidated hexa-tetrahedron.

We may also combine the hexa-tetrahedron with a cube, by uniting it to the very cube which has served for its construction. The polyhedron which results from this combination being formed by the union of a cube and a hexa-tetrahedron, I gave it the name of cube-hexa-tetrahedron; it has 32 summits and 54 faces; viz. six squares, and 48 isosceles triangles.

If we prolong in this polyhedron the plans of the twenty-four triangular faces adjacent to the square faces on the side where they join those faces, until they cut each other by fours outside of the polyhedron opposite to those squares, we shall obtain a new representative form, produced by the union of a hexa-tetrahedron, a cube, and an octohedron which will have 38 summits and 48 faces, the half of which will be equal rhombs, and the other half isosceles triangles also equal among each other. In order to designate it by a name derived from this property, which distinguishes it from all the other representative forms in which we find at once tetrahedrons and octohedrons, I shall call it amphihedron.

In order to form a simple idea of the combination of the hexa-tetrahedron with a hexahedral prism formed by the union of two regular octohedrons, we shall conceive the hexa-tetrahedron placed in such a way that two of its hexagonal faces shall be horizontal, when the middles of its six square faces will be placed as the six summits of one of the octohedrons of which the prism is composed. We may then place those six summits on the perpendiculars elevated in the midst of these faces. The six other summits of the hexahedral prism will answer to the six hexagonal faces of the hexa-tetrahedron different from those which we have placed horizontally, *i. e.* in a direction perpendicular to the axis of the prism. If we determine the respective dimensions of the two polyhedrons, so that each side of the bases of the prism shall meet the ridge of the hexa-tetrahedron, which separates those of its faces to which the two extremities of this side answer, we shall obtain a representative form composed of six tetrahedrons and two octohedrons which will have 36 summits and 50 faces; viz. two hexagons similar to those of the hexa-tetrahedron, 12 quadrilaterals, 24 isosceles triangles, and 12 scalene triangles. I shall give it the name of pentacontahedron.

In order to unite a hexa-tetrahedron with a trioctohedron, it is sufficient to place one of the three octohedrons of which the latter is composed in the same way as the octohedron which we

 have

have joined to the hexa-tetrahedron in order to change it into a pyramidated hexa-tetrahedron. The result of this combination is a polyhedron with 24 summits and 80 triangular faces. I shall give it the name of octocontahedron.

[To be continued.]

XXI. *On Fire Damps in Mines, &c.*

To Mr. Tilloch.

Sir,—I have to regret that none of the many *practical* coal masters, viewers, or agents, who are readers of your useful work, have attended to the requests that I ventured to address to them, in page 303 of your last volume, for communications on the causes and prevention of the enormous evils arising from *fire-damp* in coal mines. Since then I have read in a periodical work, to which I there alluded, a long paper by the Editor, giving a *Geognostical* Sketch of Northumberland, Durham, &c.—on which I beg to remark, that most of the facts therein mentioned, which are useful, and numerous others such, respecting the geology of this district, were already before the public, in the sections and writings of Millar, Forster, Bailey, Farey, Wynch, &c.; but of whose prior labours in the same field, not a hint escapes the learned Editor: he has however thought proper in a long *note* (written in his peculiar manner) to notice my former letter to you, referred to above; and assuming therein, without sufficient reason, that his Newcastle correspondent and myself meant to assert, that *sulphuretted* hydrogen gas occasioned the repeated explosions in Felling Colliery; proceeds to say, "Now I do not see, how *iron pyrites* can contribute to the formation of *carburetted* hydrogen;" a thing never asserted by us, but, that *probably* pyrites and *bad small coals* might *contribute* to the dangerous accumulation of inflammable gas; being well aware that a part of it, at least, issued in blowers or jets, from newly opened joints in the coal and in its roof and floor, as Mr. Buddle has so well described.

The geognostical sketch alluded to, mentions, among the late discoveries of its author in the mines of this district, that cubes of fluor spar have *been entirely removed* therefrom, without his being able to tell how or where; he says, "The change took place in the centre of the vein, a hundred fathoms below the surface of the earth, surrounded *on all sides* by walls of solid stone!, and quite impervious both to air and *moisture!*" He mentions also, icicle-like masses of *fused galena*, being found suspended in cavities in some of the veins; that in some districts (though *not in this*) a coke-like state of the coals was observed, where

where they came in contact with a *whin-dyke;* and on which knotty geognostic points, he truly remarks, they show us, that "we are much further from an accurate knowledge of the constituents of bodies, than is generally believed, and that several of our elements are undoubtedly compounds." They show us also, "that changes are still going on in the internal parts of the earth, which are totally beyond our comprehension." I ask no more of the learned Editor, than to apply these, his own maxims, *to the causes of the evolution of inflammable gas* in coal mines.

Notwithstanding the long period this Editor boasts, of having *spent in coal countries,* and his late high geognostic pretensions, he must submit to be told by practical colliers, that such *great numbers of persons* having been killed at once, on the too frequent explosions of late years in the Newcastle pits, has arisen from *the magnitude of their under-ground works,* and the small number of shafts they sink for *separate works,* owing to their great expense; and has *not been owing* to the mere circumstance of their *great depth* beneath the surface; much less so, to the extreme ignorance of the art of coal-mining, with which his "delicacy*," great as it is, has not permitted him to neglect charging on them: mixed indeed, with certain sly hints, that he himself has something new and wonderful to propose, whereby collieries may "be constructed on more scientific principles," when the funds of the new Society shall enable them, by *a premium sufficiently large,* to draw forth these new means of saving "the numerous lives *sacrificed* to the present system."

If the learned Editor would *condescend* to inquire properly of the practical men, the facts through six or eight times as many *coal-districts* as he seems yet to be sufficiently acquainted with, and to consult the published accounts of *violent explosions* in coal-pits, which have been recorded in English publications, from the earliest ones mentioned in the Philosophical Transactions to the present time, he will find a very large majority of these to have happened in pits only a third, a fourth or even a fifth part of the depth of many pits in England (some even near Newcastle, I believe) where no explosions have ever happened, and that consequently his whimsically conceived, and irremedi-

* The learned Editor says, the overseers of the coal-mines "affect mystery and concealment. Hence one is *afraid to depend* upon the information which they communicate." This may account for his own obvious deficiency in information, on a subject in which he seems so fond of obtruding himself; but let me assure him, from extensive experience among the class of men whom he thus asperses, that the very reverse is the case, and that his cause of complaint (if just?) is to be sought in *his own manner* of communicating: this at least is the explanation offered by several of his personal acquaintance.

able defect, of *depth of pit*, (in situations where such are necessary,) cannot be substantiated.

Mr. Buddle has stated, that some pits not long opened, are subject to very destructive explosions; What then becomes of our Editor's alleged accumulation of gas *through ages*, in the works of the pit? Surely he must have blundered on the notion, that the deepest pits were also the oldest? though the reverse is the case.

The geognostical sketch furnishes us with this paragraph, viz. "both the *sand-stone* and slate-clay form the roof and *floor* of coal-beds, but *the latter* much more frequently than the former." Are we to understand from this too loose description, that *sand-stone*, properly so called, was ever observed by the writer as the immediate *floor* of a coal bed? If so, he will confer an obligation on myself and many others, who have never seen the like, if he will mention the particular pit and coal bed, in as many instances as he may be able.

Before I conclude, let me remark, that however clearly the learned Editor may have himself understood, and may have acquiesced in, the positions contained in the first twelve pages of his last number, and two plates, as to "rents;" to men of my standard of intellect, the language appears not less unintelligible, than the cases that I guess the writer meant to describe, are contrary to the real appearances of *faults* in coal mines.

I am, yours, &c.

Feb. 3, 1815. AN ENGINEER.

XXII. *On the Optical Properties of Sulphuret of Carbon, Carbonate of Barytes, and Nitrate of Potash, with Inferences respecting the Structure of doubly refracting Crystals.* By DAVID BREWSTER, *LL.D. F.R.S. Edin. and F.A.S.E.**— *Communicated by the Author.*

In examining the changes which light undergoes during its passage through transparent bodies, we not only receive information respecting the properties of that mysterious agent; but we are in some measure made acquainted with the composition of the substances themselves, and with the manner in which their ingredients are combined. The optical phænomena, therefore, which bodies exhibit in their action upon light, are so many tests, to which the philosopher may have recourse, either in supplying the place of chemical analysis, or in correcting and modifying its results. A difference in the optical properties of two bodies, is generally an infallible indication of a difference in their elementary

* From the Transactions of the Royal Society of Edinburgh for 1814.

elementary principles; and whatever confidence we may place in the skill of the chemist, or in the accuracy of his methods, the mind can never rest satisfied with the results of an analysis which is directly opposed by optical phænomena.

It is highly desirable, therefore, that the chemical philosopher would avail himself more frequently of the agencies of light in the prosecution of his inquiries. The various products to which his attention is constantly directed, cannot always be preserved for subsequent examination, and can seldom be procured by the experimental philosopher. An opportunity is thus lost of confirming his own results, and of contributing most essentially to the progress of optical knowledge. It is by the alliance, indeed, of chemistry with optics, that great revolutions are yet to be effected in physics; and the time is probably not very distant, when, by their united efforts, we shall be able to develop those mysterious relations among the elementary principles of matter which hypothesis has scarcely ventured to anticipate.

In the following paper, I propose to describe the optical properties of sulphuret of carbon, carbonate of barytes, and nitrate of potash, and to illustrate the conclusions to which some of these properties lead, respecting the structure of doubly refracting crystals.

I. *Sulphuret of Carbon.*

This remarkable fluid was lately discovered by Lampadius. It is pure and colourless like water; has a specific gravity of 1·272; is remarkable for its extreme volatility; and is composed of 85 parts of sulphur, and 15 of carbon.

Owing to the great length of spectrum which this substance produces, I found considerable difficulty in measuring the mean index of refraction. By taking the bisecting ray beyond the green rays, and very considerably advanced upon the blue space, I obtained the following results:

Angle of the prism	8° 10′
Angle of refraction	5° 38′
Refractive power	1·687

By considering the bisecting ray, as passing through the green space, and near its confines with the blue, the following measures were obtained:

Angle of the prism	8° 10′
Angle of refraction	5° 27′
Refractive power	1·6632

As the sulphuret of carbon has nearly the same action upon the red and green rays, as balsam of Tolu, I have no doubt but that the bisecting ray is considerably advanced upon the blue space, and that the mean index of refraction is nearly 1·680.

A prism of flint glass, with a refracting angle of 20° 53′, corrects the colour produced by a prism of sulphuret of carbon, having a refracting angle of 8° 10′; the light being incident perpendicularly upon the fluid prism. Hence it follows, that the dispersive power of the sulphuret, or the value of $\frac{dR}{R-1}$ is 0·115, R being the index of refraction, and d the part of the whole refraction to which the dispersion is equal; and that the refractive power of the extreme red ray is 1·623, and the refractive power of the extreme blue ray 1·737.

From these experiments we conclude, that the *sulphuret of carbon exceeds all fluid bodies in refractive power, surpassing even flint-glass, topaz and tourmaline; and that in dispersive power, it exceeds every fluid substance, except oil of cassia, holding an intermediate place between phosphorus and balsam of Tolu.*

These relations will be better understood from the following short tables:

Refractive Powers.

Sulphur, native	2·115
Boracite	1·701
Sulphuret of carbon	1·680
Tourmaline	1·668
Blue topaz	1·636
Flint glass	1·616

Dispersive Powers.

Oil of cassia	0·139
Sulphur	0·130
Phosphorus	0·128
Sulphuret of carbon	0·115
Balsam of Tolu	0·103
Flint glass	0·052

Although oil of cassia surpasses the sulphuret of carbon in its power of dispersion; yet, from the yellow colour with which it is always tinged, it is greatly inferior to the latter, as an optical fluid, unless in cases where a very thin concave lens is required. The extreme volatility of the sulphuret is undoubtedly a disadvantage to which the oil is not liable; but as this volatility may be restrained, we have no hesitation in considering the sulphuret of carbon, as a fluid of great value in optical researches, and which may yet be of incalculable service in the construction of optical instruments. All other fluids are separated from these two, in their optical properties, by an immense interval; and hence we are of opinion, that oil of cassia will yet be found to consist of ingredients as remarkable as those which enter into the composition of sulphuret of carbon.

II. *Car-*

II. *Carbonate of Barytes.*

The native carbonate of barytes possesses, like the agate, the remarkable faculty of forming two images, one of which is bright, and the other nebulous. The shapeless appearance of the agate; its heterogeneous and imperfect structure, and its anomalous character in the mineral kingdom, corresponded well with the singularity of its optical properties, and discouraged the anticipation of analogous phænomena, in minerals of a more perfect structure. I was, therefore, surprised to find the same character in carbonate of barytes, a mineral which has a regular crystalline form, and possesses two distinct refractive powers. The index of refraction for the perfect or least refracted image is 1·540; and its dispersive power 0·0285.

In order to observe with accuracy the phænomena presented by the carbonate of barytes, I formed nine prisms, cut in different directions, from the same specimen. In one of these prisms, which was bounded by planes parallel to the striæ or longitudinal joints, the least refracted image was extremely distinct, while the other was a faint nebulous image, of a brownish-red hue. It was small and round, and the intensity of its light was inconsiderable, when compared with that of the bright image.

When the image of a candle polarized by reflection, was viewed through this prism, having the longitudinal joints parallel to the plane of reflection, the light which formed the bright image of the candle was wholly reflected, while the nebulous light alone penetrated the mineral. But when the longitudinal joints were perpendicular to the plane of reflection, the light image became extremely distinct, in consequence of the nebulous light having now refused to penetrate the prism.

In a second prism, the nebulous or most refracted image was more luminous than in the first, and approached to a definite form; the general shape of the candle being distinctly visible.

In a third prism, the nebulous image was more luminous than in the last case, and the form of the candle still more distinctly seen; but it had now the appearance of an assemblage of incoincident images.

In a fourth prism, in which the plane of refraction was parallel to the longitudinal joints, both the images were imperfect, and the most refracted image was extremely faint. By inclining the prism, an image appeared on each side of the least refracted image; but they were polarized in the same manner, and were probably analogous to the two images which are frequently seen in specimens of calcareous spar.

In a fifth prism, which was formed by planes nearly perpendicular to the longitudinal joints, *four* images were plainly visible, all of which were imperfect, and consisted of circular arches of nebulous

nebulous light. The two middle ones, which were the principal images, were equally luminous, and were polarized in an opposite manner, like all other double images; but each of the two outer images was polarized in the same manner as the bright image furthest from it. The *most refracted* of the two principal images was in this case the more perfect of the two, and exhibited a degree of prismatic colour so much greater than the other, that it obviously belonged to a higher dispersive power. When the light enters the prism, and emerges from it at equal angles, the four images are not distinctly separated, and are extremely imperfect. When the angle of incidence at the first surface of the prism is increased, the images become more and more distinct, and better separated; but, by diminishing the angle of incidence, all the images approach one another, and are confounded into one mass of nebulous light.

With a plate of carbonate of barytes, which was about two-tenths of an inch thick, and which had its surfaces at right angles to the direction of the longitudinal joints, the image of a candle was a large circular mass of light, when the incidence was perpendicular. By inclining the plate, this mass was changed into an annular image: by increasing the inclination, it assumed the form of a crescent; and at a considerable angle of incidence, it was separated into three imperfect images, or circular arches of nebulous light, similar to those which were seen with the fifth prism. The middle image, which was the brightest, consisted of the ordinary and extraordinary image, which were not separated, in consequence of the parallelism of the refracting surfaces. In one position of the plate, these arches were crossed by other three similar arches, inclined to the first at an angle of 10° or 12°.

The phænomena which have now been described, differ in several respects from those which are presented by the agate. In the *carbonate of barytes*, the two images are distinctly separated, and are, therefore, formed by two separate refractive powers; whereas in the *agate*, the bright image is placed in the centre of the nebulous mass. In the *carbonate of barytes*, the imperfect image occupies a small space; but in the *agate*, it is an elongated mass of light, extending about $7\frac{1}{2}°$ in length, and about 1° 7′ in breadth, on each side of the bright image. These differences, however, are probably owing to the different ways in which the two minerals have been cut; but it is not easy to submit this point to direct experiment, on account of the difficulty of procuring a mass of agate, from which a variety of transparent prisms could be obtained. It follows, however, from the theory of the depolarization of light, which I have explained in another place, and which is supported by all the evidence which

which any theory can possess, that the specimens of agate which depolarize light must necessarily form two distinct images,—a phænomenon to which we have found a rapid approximation in the carbonate of barytes.

The property which has now been explained, forms a simple and infallible mineralogical character of the striated carbonate of barytes; and is particularly valuable to those who have been perplexed by the numerous marks with which some writers have laboured to distinguish it from its kindred minerals. The assistance, indeed, which optics afford in discriminating minerals, is of the most extensive kind; and it is much to be wished, that mineralogists would exchange many of their vague distinctions for those unambiguous characters which bodies exhibit in the modifications they impress upon light.

The Abbé Haüy has, in some measure, begun this reformation, and has set a brilliant example of what may be effected by the aid of mathematical and physical acquirements. In his admirable work on Crystallography, which has never been duly appreciated in this country, he has created a new science, in which he has shown how to determine the integrant molecules of crystallized bodies; and how, from a few primitive forms, may be derived that endless variety of secondary crystals which adorn the mineral kingdom. The recent discoveries which have been made in optics, enable us to give a new direction to these interesting inquiries; to determine the forms and even the angles of crystals from their optical properties; and out of a mass of shapeless fragments, to reconstruct an artificial crystal, of which all the parts shall have the same relation as they had in nature to the axes and sides of the primitive crystalline form.

III. *Nitrate of Potash.*

This salt possesses the most remarkable optical properties of any crystal that is at present known, and its various actions upon light are of the most anomalous and instructive character.

The crystals which I employed were all equiangular hexaedral prisms; and the light was transmitted through two natural faces, separated by another face, so that they were inclined to each other at an angle of about 60°. This inclination is by no means convenient for measuring refractive and dispersive powers; but I attempted in vain to form artificial faces inclined at a less angle, and those means which I had found successful with other soft crystals, completely failed when applied to this salt.

When a candle was viewed through the nitrate of potash, I observed a double refraction very much greater than that of calcareous spar,—a phænomenon which gave me the more surprise, as the Abbé Haüy, who examined many splendid crystals of this salt, ascribes to it the property of simple refraction.

The

The least refracted image was a circular mass of white nebulous light, condensed at its centre into a very faint image of the candle, but without any strong prismatic tinge; while the light which had suffered the greatest refraction, formed a distinct and highly coloured image. The great interval between the two images; the achromatic nebulosity of the first, and the distinctness and deep colours of the second image, formed altogether a singular phænomenon, and, at the same time, afforded an *ocular demonstration of the existence of two dispersive powers in doubly refracting crystals.*

The following measures of the refractive powers of the two images were taken with the greatest care:

Angle of the prism	60° 21′
Angle of refraction for the 1st image ..	24° 8′
Angle of refraction for the 2d image ..	38° 54′
Index of refraction for the 1st image .	1·3374
Index of refraction for the 2d image ..	1·5156

In order to confirm these results, I formed a new prism, and obtained the following measures:

Angle of the prism	62° 12′
Angle of refraction for the 1st image ..	24° 48′
Angle of refraction for the 2d image ..	40° 39′
Index of refraction for the 1st image ..	1·3326
Index of refraction for the 2d image ..	1·5134

By taking a mean of these results, which are extremely near to each other, we obtain for the

Least refractive power	1·3350
Greatest refractive power	1·5145

Hence it follows, that the least refraction of nitrate of potash is almost exactly the same as that of *water* which is 1·3358, —a result of such an extraordinary nature, that I felt it necessary to confirm it by repeated observations.

In measuring the dispersive power of this salt, we cannot expect the same accuracy of result on account of the great angle of the prism. Owing to the nebulosity of the first image it is impossible to measure its dispersive power; but it evidently corresponds with its low power of refraction. In order to correct the dispersion of the second refraction, it requires a prism of flint glass with an angle of nearly 60°. With an angle of 66°, the dispersion is more than corrected; but with an angle of 56° the correction is not nearly completed. The dispersive powers due to these different angles are contained in the following table:

Angles of the flint glass prism		Dispersive powers
	66°	0·0613
	60	0·0573
	56	0·0546

By

By taking a mean between the two extreme observations, we obtain 0·058 for the approximate dispersive power,—a result which could scarcely have been anticipated from the substances which enter into the composition of nitre. The following table will show the relation which this measure bears to the dispersive powers of other bodies:

Sulphate of lead	0·060
Nitrate of potash, 2d refraction	0·058
Flint glass	0·048
Water	0·035

In order to examine the character of the rays which form the two images, I polarized the light of a candle by reflection from glass, and viewed it through two of the parallel faces of a hexaedral prism of nitre. When the edges or common sections of its faces were parallel to the plane of reflection, a bright image of the candle was seen in the middle of a mass of nebulous light, exactly similar to what happens in the agate when its veins are parallel to the plane of reflection. But upon turning round the crystal of nitre, the bright image gradually vanished, while the nebulous light increased; and when the edges of the crystal were perpendicular to the plane of reflection, the bright image was extinguished, and the nebulous light a maximum. When the reflected image of the candle is viewed through two inclined faces of the nitre, the two images vanish alternately, like those formed by all doubly refracting crystals.

A prism of nitrate of potash, having its refracting surfaces equally inclined to the axis of the hexaedral crystal, possesses the faculty of depolarizing light; and hence it follows, from the theory of depolarization, that the prism must, in this case, form two distinct images.

The two neutral axes of this salt are parallel and perpendicular to the sides of the hexaedral prism; and the depolarizing axes are parallel to the diagonals of the square base common to the two pyramids which compose its primitive rectangular octaedron. The least refracted image is that which is produced by the extraordinary law of refraction.

The beautiful coloured rings which I exhibited to the Society, as produced by the action of topaz upon polarized light, and which I have also discovered in the agate, and in a great variety of other bodies*, exist also, but in a very singular manner, in the nitrate of potash.

By comparing in a rude manner the coloured rings formed by different bodies, with the thickness of the plates by which they were produced, I concluded that the conjugate diameters of the

* See Phil. Trans. Lond. 1814, part i. p. 218.

rings

rings were nearly as $\frac{1}{(m-1)^3}$, m being the index of refraction. In the nitrate of potash, however, their magnitude is quite anomalous, as it produces along the axis of the hexaedral prism a series of miniature rings, nearly *eight* times less than they should have been according to the preceding law. The beautiful generalization of the phænomena of coloured rings, which we owe to the genius of the celebrated Biot, may perhaps afford an explanation of this apparent anomaly.

The *carbonate of potash* forms also two images, one of which is bright, and the other nebulous. They are polarized in an opposite manner, like those formed by the nitrate of potash, but the nebulous image is more distinct in the carbonate. With a prism bounded by natural faces, and having a refracting angle of 49° 53′, I obtained the following measures of its mean refractive power:

Index of refraction for the nebulous image .	1·379
Index of refraction for the bright image ..	1·482

IV. *On the Structure of doubly refracting Crystals.*

Notwithstanding the numerous discoveries which have recently appeared respecting the polarization of light, no attempt has been made to apply them in solving the problem of double refraction. They furnish us, indeed, with a variety of beautiful phænomena, analogous to the polarization of light, which always accompanies the production of two images; but they afford no ground of conjecture respecting the separation of the pencil into two parts.

When I discovered the property possessed by the agate, of forming a bright and a nebulous image, and of polarizing them in an opposite manner, like all doubly refracting crystals, I was sufficiently aware of the conclusions which it authorized*; but as no other crystallized body exhibited analogous phænomena, I contented myself with stating these conclusions as mere conjectures, which required the sanction of numerous experiments.

In the carbonate of barytes, however, and in the nitrate and carbonate of potash, we are presented with properties analogous to those of the agate, and are therefore enabled to resume this subject, with that confidence which can only be derived from multiplied observations.

When we examine the two images formed by calcareous spar and other perfectly transparent crystals, we find that they have the same magnitude, and are equally luminous and distinct. There is, therefore, no circumstance which can lead us to suppose, that the light which forms the one image passes through a

* See Phil. Trans. Lond. 1813, part i. p. 101.

part

part of the crystal, having a different structure from that which transmits the light of the other image. In the carbonate of barytes, however, where the transparency of the crystal is imperfect, one of the images is nebulous and imperfect; and as the same phænomenon is exhibited in the agate and in the imperfectly transparent crystals of the nitrate and carbonate of potash, we are entitled to conclude, that the light which forms the imperfect image is transmitted through the imperfect structure; while the light which forms the bright image is transmitted through a structure of a more perfect kind. The imperfect transparency, therefore, of the crystal, and the nebulous character of one of the images, can be considered in no other relation than that of cause and effect.

From the optical properties of the agate, this conclusion derives a still higher degree of probability. The two images formed by this mineral are not similar to each other, like those of calcareous spar, though they possess exactly the same properties. One of them is bright and distinct, and the other is a mass of nebulous light. Now it happens that the agate possesses two different kinds of structure, corresponding to the characters of its two images, and distinctly perceptible even to the naked eye. One of these structures is composed of small serpentine lines like the figures 3333, resembling the surface of water ruffled by a gentle breeze; and I have a specimen in my possession, one-half of which has much larger serpentine lines than the other. The light which passes through the serpentine lines, is that which forms the nebulous image; while that which passes between them forms the distinct image. This may be demonstrated by a variety of experiments.

When the light is transmitted through a part of the agate that has the largest serpentine lines, the nebulous image has an appearance different from that which it has when the light is transmitted through the other part where the serpentine lines are smaller. If the agate is inclined in the direction of the serpentine lines, so as not to prevent the rays from passing between them, the bright image will be distinctly visible as before; but when the agate is inclined in a direction at right angles to this, so as to prevent the rays from passing between the serpentine lines, the whole of the transmitted light is nebulous. Hence it follows, that the nebulous image is produced by the imperfect structure of the agate, indicated by the serpentine lines; while the bright image is produced by a structure the same as that of all other transparent bodies.

The curvature of the nebulous light, in some specimens of agate with incurvated veins, and its constant parallelism to the laminæ,

laminæ, and to the direction of the serpentine lines, give additional probability to this conclusion.

Here, then, we have a case of the most unequivocal kind, in which one image of a doubly refracting crystal is produced by one structure, or by one part of the crystal, while the other image is produced by another structure, or another part of the crystal; and hence we are led to conclude, in general, that the two images exhibited by all doubly refracting bodies, are formed by two different structures, related to some axis or fixed line in the primitive crystal. Whether this difference of structure is produced by a difference in the arrangement of the elementary molecules, or is owing to a combination of different ingredients, is a point which still remains to be determined.

The phænomena presented by the agate and the carbonate of barytes, convey still further information respecting the structure of these imperfect crystals. In one direction, the light transmitted by the agate is wholly nebulous; the perfect image being converted into a shapeless cloudy mass, and confounded with the nebulous image. In another direction, one of the images is distinct and perfectly formed; and in one specimen, which has the faculty of depolarization, there must necessarily be two perfect images. In a prism of the carbonate of barytes, both the images were imperfect. In a second prism, the one image was nebulous, and the other distinctly formed; while, in other prisms, there was a rapid approximation to two perfect images. Hence it follows, that the imperfect structure, which, in general, transmits only a mass of nebulous light, allows a distinct image to be formed, when the rays are incident in one particular direction; while the perfect structure, which in general gives a distinct image, allows an imperfect image to be formed, when the light penetrates it by a particular path.

These inferences, which I conceive to be irresistible, have a higher degree of importance than we may at first be disposed to attach to them. They form a real step in the explanation of double images, and indicate a part of that structure which is necessary to their formation. The other phænomena of double refraction are still involved in obscurity. The opposite polarization of the two pencils may be explained by supposing the crystal to consist of laminæ inclined in various directions; and, as I have shown in another place *, the same phænomena may be actually produced by an artificial crystal composed of bundles of glass plates. The most perplexing point, however, is the extraordinary refraction which takes place at a perpendicular incidence. Whether this phænomenon is the result of an extraor-

* Phil. Trans. Lond. 1814, part i. p. 230.

dinary law of refraction, as Huygens and Newton supposed, or is produced by forces dependent on the elementary structure of the crystal, is a question which still remains to be determined. The extraordinary reflection and refraction arising from the last of these causes, which I have discovered in mother of pearl*, present an analogy, by no means remote, to the phænomena of double refraction.

XXIII. *Of the Physiology of certain Disorders of Health founded on a Knowledge of the proportionate Development and Functions of the special Organs of the Mind.* By Thomas Forster, *Esq.*

To Mr. Tilloch.

Sir,—In a former paper I endeavoured to point out some of the practical applications of the new discoveries into the structure and functions of the brain, with a view to show the various means in which it might become useful to society. For I consider every inquiry as to the particular utility of any science to be merged in the general question, Whether or no it be the illustration of truth? Yet it is of use to particularize the special purposes for which it may be employed. I proceed to mention briefly a few curious facts relative to the history of certain kinds of aberration of mind, and to their connexion with particular organization.

It seems to me, that we cannot obtain any accurate knowledge of the nature of this mental disorder, till we have first become acquainted with the mind in its healthy state. Insanity consists in the erroneous or inordinate energy of the functions of the mind in general, or of some of the faculties in particular. To learn the physiological history of these, we must previously know what are the primitive faculties of the mind in a healthy condition; we must first know the passions, learn what are the results of primitive and uncompounded organs, and what are compounded

* This substance, whose remarkable optical properties I have explained in another place, resembles the agate, the carbonate of barytes, and the nitrate and carbonate of potash, in giving a bright and a nebulous image, when the light is transmitted in one direction, and two bright images, when the light is transmitted in another direction; but it possesses this property under circumstances of such an extraordinary nature, that I could not with propriety have introduced any account of it into this paper.

A number of soft substances, of animal and vegetable origin, have likewise the faculty of forming a bright and a nebulous image, under various singular modifications. A full account of the results which I have obtained with this class of substances, will be found in another paper.

of

of two or more separate faculties; what are the mutual influences of the different faculties; and lastly, what external stimuli, or what particular stages of digestive disorder and bodily ill-health in general, excite particular sentiments and propensities of the mind more than others. At present our knowledge of these data is very imperfect; and consequently our view of the aberrations of mind is very limited, and the little knowledge gained from the experience of apparently incongruous and unarranged particulars forms as yet the basis of the medical treatment of maniacs. My own very limited knowledge of their diseases prevents me from applying the knowledge of the organs and functions of the brain, which I have obtained from continued observation, to the elucidation of madness on such an extensive scale as might be practical for many others, whose experience of the conduct and history of madmen might enable them to be of great service in elucidating their sort of diseases; if they would patiently investigate the facts whereon the new views of the cerebral functions are established. I shall proceed to mention only a few considerations of some common disorders of mind, which reflection and observation of the connexion between the forms of the brain and the disorders it occasions the subject of have suggested. We cannot describe manias without adverting to the passions of which they are frequently exaggerations, differing also in the mode of the affection. Maniacal fury, for example, seems a disordered action of the organ of combativeness, more or less catenated with disorders of other organs. When the organ of destructiveness is much excited in madness, it causes many of those horrible murders and acts of destruction committed by the insane, of which cases are familiar to all keepers of them. Pride is often strongly excited in madmen, when the organ of haughtiness is largely developed. In short, where there is a decided character in the mania there is a large development of the corresponding organ. There are instances of men mad in the organ of benevolence, who give away all to the poor. In the organ of religion there are innumerable maniacs, as there are likewise in mysticism. The great development then of particular organs points out the particular nature of many lunatics; while ideality produces the strange imaginations of all. There is the late well known instance of a person mad from constructiveness, who imagined machines constructed under ground to destroy him. Madness too may be only on one side of the head in some cases; as in that related by Dr. Spurzheim. I merely hint at those things now, for subjects of future observation. I shall conclude with an observation I have made on melancholy and hypchondriasis. Melancholy seems to be a disordered action of the organ of cautiousness; we always find it accompanied with fear and

and anxiety of some kind; and I have noticed that those persons who are subject to fits of this disorder have that organ much developed. I have indeed observed this to be the fact in cases too numerous to doubt of the connexion between the two circumstances. The fear of death, the fear of hell, of dæmons, of accidents, &c. are the result of this organ; education, external impression, and the mutual influence of other faculties modify the affection, and determine the object dreaded: but the intrinsic character of the disease is the same; fear and anxiety are its characteristic marks. In hypochondriacism, the organ of ideality suggests the whimsical and delusive ideas, for fear, or the organ of cautiousness, to act upon. But as persons thus organized are not always hypochondriacal and melancholy, some state of bodily ill-health must be necessary to the production of the disease. I think the opinion of antiquity, the sanction of ages, and facts well known to modern practitioners, warrant the notion that disorders of the organs of digestion, and particularly hepatic irritation, exercise in general their morbid feelings. If so, the disorder of the chylopoietic viscera must have a special sympathy with the organs of ideality and cautiousness; for, if they merely excited the brain in general, the organs of the reflective powers would have a commensurate action, and the erroneous ideas would not prevail; which is far from being the case. The reflecting organs grow weaker, and at last the patient ceases to reason against feelings at first known by the reasoning powers to be fallacious. Sometimes, when the organ of individuality is weak from smallness and disease, the patient fancies that external bodies have no real existence: when it is too large and diseased, he fancies he has heard every new thing before. In other cases of defective individuality, I have known single consciousness destroyed, analogous to double vision. These and many other facts are curious, and lead me to regard individuality as concerned in single consciousness, and other functions attributed to the commissures of the brain alone. For it seems to me that the commissures cannot be the cause why we conceive one object to exist externally, and to excite vision and to act at the same time; and there seems to me a great analogy in the function of mind whereby we conceive unity of existence from double impressions of the same organ, and that whereby we identify the sensations of different organs, and regard them as qualities of one external body.—Little however, indeed, is known about these things at present: but, little as it is, it must be studied and improved on. Reasoning only on theories, and a blind respect for the doctrine of the schools, have already caused metaphysics to appear absurd, philosophy in general vague, and medicine contradictory or empirical. We must follow Nature, and not those

 who

who have previously sought her in vain. I only wish to excite others to lay aside, as I have done, the notions of the old philosophers, to discard all previous opinion, and to reason only on facts which can be demonstrated. I am contented when these facts are pointed out to me by the greater industry of those who are travelling, like myself, by short stages, from the bottom to the top of a mountain.

Yours, &c.

THOMAS FORSTER.

XXIV. *Dr.* SPURZHEIM's *demonstrative Course of Lectures on Drs.* GALL *and* SPURZHEIM's *Physiognomonical System.*

[Continued from p. 63.]

Lect. 9. CERTAIN functions are mediate, others immediate.

XXIV. *Organ of space.* This teaches the relative position of objects to each other, and enables us to recollect places. Gall is so deficient in this faculty that he often forgets the particular apartment of his patients, where there are, as in Vienna, many inhabitants in one house. This organ consists of two elevations over the eyebrows at their inner end. Astronomers, travellers and voyagers have this organ highly developed, as in Newton, Captain Cook, &c. Animals also possess it, and find their route from very remote places. Instances of dogs carried in coaches from Vienna to Petersburg, and to London, from Paris to Marseilles and Naples, and yet they found their way back to their original homes. These facts are inexplicable on the supposition that animals smell the paths of their masters. Cats have likewise been known to travel several miles and carry their kittens. Pigeons were taken from Liege to Paris and suffered to escape: one returned to Liege in two hours, another was a day, and a third returned after three days. Some authors have considered this a sixth sense. Swallows, storks, quails, starlings, nightingales, &c. These fowls not only come back to the same climate and country, but to the same window, bush, chimney, or tree. These migrations are not from want of food, as they frequently occur when food is abundant, and certain birds often arrive in certain climates before their proper food is produced. Nightingales in cages become uneasy and disquieted at the period of emigration; which proves an internal feeling. This faculty makes the traveller, geographer, landscape painter; it recollects localities, judges of symmetry, measures space and distance, and gives notions of perspective. The primitive faculty is space, as all things have localities.

XXV. *Or-*

XXV. *Organ of order* is an elevation at the outer extremity of the eyebrows. Dr. S. has discovered this organ since his arrival in this country, and consequently its position and functions are not sufficiently determined. It induces habits of regular arrangement, system, &c., and is more common in England than in any other country. Cleanliness belongs to this faculty; but, says the Professor, observe the fact and the organ, and then decide.

XXVI. *Organ of time.* Some persons are fond of chronology, others never think of it. There is more connection between order and number than between time and number, and more between time and order than time and number; order has a relation to objects, time to events or facts; the organ of time therefore is situated above that of colour, and laterally over that of space.

XXVII. *Organ of number,* or of mathematics; this organ consists in the external angle of the eyebrow being lower and more full than the internal. Children of five years old often manifest this faculty, which relates to number, unity, and calculation. Gall called it the faculty of mathematics; but it does not appear to extend beyond arithmetic and algebra, as geometry and other branches of mathematics require the organs of form, size, colour, &c. It is well developed in Newton, Euler, the late Mr. Pitt (a financier), and Jedediah Buxton, who amused himself by counting the number of words Garrick spoke on the stage during a night's performance. Negroes are very deficient in this faculty, many of them being able to count only to five. Magpies, it is said, can count only three; but Dupont of Nemours alleges they can enumerate nine.

XXVIII. *Organ of tune;* neither ear nor voice is sufficient for musical talent; men hear as well as women, yet the latter sing; animals hear better than man, but do not sing. Some persons almost deaf have musical talents; others have a good voice and no musical talent, while others again have musical talent and no voice. Birds sing without instructions. This organ gives the forehead a square appearance, as it forms an angular ridge from the temple to the top of the forehead, and the eyes are slightly depressed inwards.

XXIX. *Organ of languages.* Gall, observing that those with large prominent eyes, especially if below them there appeared a swollen circle, like that in the heads of Locke, Milton, Voltaire, &c. had always a good verbal memory, supposed that there were two organs, one of words and the other of languages. It is true, some persons easily acquire the spirit of different longuages without having a great memory of words, and others easily learn words without knowing the spirit of any language. Nevertheless, Dr. S. thinks this but one faculty. Judgement he considers

only a mode of action, as pleasure and pain are modes of feeling, every faculty having a good or a bad mode; hence he concludes that the memory of words and philology in general originate in the same faculty. If in a skull we find the sockets of the eyes deep, it proves that the brain had not been large there; on the contrary, if the sockets be shallow, it indicates the greater development, the brain having literally pushed the eye-balls forward. Here Dr. S. took a masterly review of the question respecting the influence of words on ideas. In France the opinion has received academical rewards, that words give ideas, and that persons are incapable of thinking without words! But have animals a language? The term idea is ambiguous, some taking it according to its etymology, others metaphorically. Dr. S. calls every conception of the knowing faculties an *idea,* and *reflection* every function of those faculties which compares. Every faculty exists by itself; we feel without knowing the name of the feeling; we perceive external objects without knowing that the medium is light: therefore the idea exists without a word. Propensities and feelings exist without the knowledge of words; these only serve to communicate a knowledge of them to others. Individuals communicate their feelings and sentiments to each other; one uses artificial, and another natural language. The feeling of anger always shows itself, and is intelligible even in man to animals; it is the natural language, and is consequently intelligible to every being capable of similar feeling A dog knows when his master is angry, because he possesses the same faculty: but he cannot understand the natural signs of adoration to God; yet dogs have sometimes known a few things in three languages. This natural language is common to all animals; the young cry, the mother hears it and gives them food; this is known by the difference in the sound of the voice; the male calls the female; hence animals have a natural language. Artificial language is peculiar to man; he observes, compares, and imitates; animals cannot imitate their sensations or multiply their enjoyments; man only makes a language; the same faculty enables him to produce fire, make clothes, &c. The voice is the general means, the senses are only the media; the dumb use gestures, the deaf want only one sense. The organ of language therefore is the capacity to learn signs; some cannot learn languages; it is a partial idiotism; others acquire only sufficient knowledge of abstract signs to form single words. Some hydrocephalic patients have the eyes prominent, the same as if the organ of language was highly developed, and are nevertheless destitute of memory. On the other hand, some persons have been known to speak without a tongue.

[We have to apologize for the omission of the preceding Lecture in its proper place, from its having been mislaid.]

Lecture 16. The termination of education is in civil and criminal legislation. Those who form institutions should know man, otherwise they cannot be permanent. Have then physiognomical inquiries a tendency to influence legislation? What is legislation? Subordination of the faculties; one must be submitted to the others. Very few act morally or do good merely by love, but great numbers do by law. Legislation if defective cannot be universal, and there can be no contradiction between divine and civil legislation; the happiness of this world is not contrary to that of the next. All faculties proper to man are the foundation of legislation; it must be just, charitable and firm; if only charitable, the aim is not fulfilled; if severe, we also fail. What is the aim of criminal legislation? Certainly not revenge; but, 1st, to prevent crimes; 2d, reform criminals; and, 3d, secure society against incorrigible malefactors. How are crimes prevented? We punish, but it does not prevent them; preventing crimes in adults is the continuation of education. Certain punishments are not feared, and are therefore useless; education is more powerful than punishment; idleness, laziness, and sometimes the laws create crimes, as some laws are not against crimes, but are only peculiar perceptions of some minds. Educate and keep down the animal, and confine incorrigible criminals; but how? The houses of correction in their actual state are houses of perversion. If children be taught or treated as animals are, they must also be perverse. Suspected persons and children now often live in prisons with the greatest criminals, where, being idle and ignorant, they learn nothing but how to commit crimes. If men are designed to be honest, treat them so; educate and enable them to earn a living rather than to grow worse. Many charitable institutions are positive rewards for crimes; criminals should be corrected, but not rewarded; such arrangements are unnatural, and must be temporary. Bentham's ideas of criminal legislation are very good, so also are the institutions in Philadelphia. Correction is the end and aim; and until criminals are corrected, they should not be allowed to be at large in society. It is very unjust to punish all crimes with the same punishment; death does not prevent crimes; legislation should think of other means. It is difficult, indeed, but should be studied. Some criminals are incorrigible and guilty, others incorrigible and innocent; the former must be punished, the latter being idiots must be confined. Idiots may have some parts of mind, but not a will, and are consequently not to be punished; there are idiots in propensities and in sentiments, as

 well

well as in intellects; some of them have a talent for music, and Cretins have had constructiveness. Diseases of the mind intermittent; and hence partial insanities which are difficult for a judge to recognise. Absolute and individual justice; the former results from the sentiment of justice united to the superior faculties, the latter is followed by legislators. St. Augustin observes, The letter kills, the spirit preserves. Grief weakens the faculties more than passion, and the strongest feelings may be perfectly quiet. Here Dr. S. took a survey of the different crimes of murder, suicide, and infanticide*: the latter, he observed, was the only crime on which men legislated without being capable of a condition to commit it, and consequently they should proceed with great caution. Such crimes are often occasioned by diseases of the mind. This term the Professor considers unphilosophical; the derangement is in the body and organization; the mind cannot manifest itself without an organ, as we cannot see without eyes. In future we must consider the diseases of the brain like those in the other parts of the body, and that some of them are curable and others incurable. Such diseases must be divided according to their cause, and receive a physical and moral treatment. The propensities are to the brain what food is to the stomach, air to the lungs, or light to the eye; they must not be aggravated or indulged to excess, any more than excess to any other organs. If the brain of children be too large, then do not exercise their minds too much; the mind sometimes increases so that children die. Irritability is periodical; friends or persons of similar dispositions experience this feeling at the same time all over the world, in Vienna, Paris, or London. All phænomena have periodicity; plants increase more in spring, and children grow more at one period than another. Every one feels a *noli me tangere* once a month, when he wants appetite, is disgusted and peevish with every thing. Some persons experience this twice every 28 days; then it passes off, and they become cheerful as before. Medical men then say they have cured such a person, but nature has done it. Derangement occurs at certain periods, when suicides are more numerous; these attacks can be anticipated. To cure such diseases, the period must pass, and then the irritability subsides. Thus, observed Dr Spurzheim, in his concluding lecture to an audience of British philosophers, I have demonstrated the truth announced by Charles Bonnet, that "the moral nature of man is discoverable by his organization."

* This crime might be almost entirely banished from society, were the fathers of murdered children always to be tried by the laws of the land, and subjected to a certain punishment, according to the degree of guilt which an impartial and enlightened jury might determine.

XXV. *On*

XXV. *On the Rules for Algebraical Multiplication. By Mr.* J. Dillon.

To Mr. Tilloch.

Sir,—Having seen in your Magazine for January some remarks by Sir H. Englefield, tending to explain the algebraical theorem, at once so necessary for the young mathematician to master, and yet so difficult for him fully to understand, that a *negative* quantity, multiplied into a *negative* quantity, gives a *positive* result, I beg leave to add a few observations which have occurred to me upon the subject, and which may perhaps in some degree tend to place the matter in a clearer point of view.

The signs + and − appear too generally (at least in elementary works) construed to mean *plus* and *minus;* a sense which, though perhaps always included in, does not appear to constitute the whole of their definition. The sign + signifying, in fact, that the term to which it is prefixed is *positive*, and the sign − that such term is *negative*, that the one should be *plus*, (or the object of addition,) and the other *minus*, (or the object of subtraction,) when addition or subtraction with other quantities is in question; these are rather *consequences* flowing from, than essential parts of the nature of, such signs of + and −.

The fallacy of considering + and − as merely meaning *plus* and *minus*, will plainly appear where multiplication or division is intended, as $-a \times b$, or $\frac{-a}{+b}$, where it is evident neither *plus* nor *minus* can be meant by the signs + and −; and it is in this fallacy, as it appears to me, that all the difficulties of the present question have their origin; for, by always affixing the sense of *positive* and *negative* to these signs, nearly all the obstacles which impede the progress of the learner on this subject will vanish.

I scarcely need previously to observe, that the algebraist is as conversant with the idea of a negative as of a positive quantity. Considerable confusion appears, however, to have arisen from attempts to render this idea familiar to minds not accustomed to abstract reasoning. Thus, therefore, it is frequently represented that $-a$ is not so much the negative quantity a, as it is the positive quantity a with a mark affixed to it, signifying that it is to be subtracted from some other quantity either actually known, or to be discovered; whereas, in fact, it should be considered as strictly a *negative* quantity, capable of destroying or counteracting a positive quantity of equal value, when it comes in contact with such, and existing in the mind in a way perhaps somewhat similar to the ideas of darkness, silence, or vacuum, which

which may be considered as abstract *negative* ideas, expressive of the absence of light, sound, or matter.

To apply these premises to the subject under consideration, namely, the multiplication of algebraical quantities, under all the varieties of the signs $+$ and $-$: of this multiplication there are three cases.

1st. When the terms are both positive, $+ \times +$.

2d. When only one of the terms is positive, $- \times +$.

3d. When both the terms are negative, $- \times -$.

For the *first* then, to take an instance, $+ a \times + b$; we have only to remember the well known principle that the multiplier is merely an abstract quantity, expressive of the number of times the multiplicand is to be added within itself, and we shall immediately perceive that the result must be $+$, as it is merely proposed that $+ a$ should be taken b number of times, without any alteration of the signs, which are indeed expressly *affirmed* by the sign $+$ affixed to b.

For the second case, $- a \times + b$; it is equally evident that $- a$ taken b number of times must on the other hand always remain $-$, whatever may be the value of b.

For the third case, $- a \times - b$, where the difficulty is supposed to rest, it may be previously remarked that if $- a$ when multiplied (as in the second case) by b or $+ b$ gives a *negative* result, then may it beforehand be expected that this same $- a$ when multiplied by $- b$ will give a contrary, that is a *positive* result.

It is a well known position in logic that two negatives make an affirmative; to say that a thing is *not not* so, is in fact but a more circuitous manner of saying that it *is* so, and exactly this process appears to take place in the case before us. The result of $- a \times - b$ is $+ ab$ for this reason; $a \times b = ab$, and the negative sign of the a is (if I may so express it) itself *negated* by the negative sign of the b. The quantity a had, we must suppose, become negative by some previous process; the *reversal* therefore of this sentence of negation must be as necessarily the consequence of its being multiplied into a *negative* quantity, as the continuing subject to that sentence would have resulted from its being multiplied into an affirmative or *positive* quantity: in other words, the sign $-$ prefixed to the b is, in fact, the *negation* of the sign belonging to a, (the quantity to be multiplied,) whether the sign of such quantity be positive *or* negative

The origin of the error, and the consequent existence of the difficulty in question, appear to be this: that the affirmation of a positive quantity, (as $a \times + b$,) and the negation of a negative quantity (as $- a \times - b$,) are supposed to be the contrast, or antithesis of each other; whereas, in fact, so far from being opposed,

posed, they are but the same thing stated in other words; or rather they tend, by different methods, to a similar result: the *real* contrast will be found to exist in the affirmation of a positive quantity, (as $a \times +b$,) and in the affirmation also (*not* the negation) of a negative quantity, (as $-a \times +b$;) and this contrast is exemplified in the contrary results of the first and second cases stated above.

Two observations only, in the way of elucidations, further occur to me: it should be remembered that in the multiplication, as in other processes of algebra, the signs only affect the signs, and the quantities the quantities: in the multiplication $-a \times -b$ to perceive the separate effect of the one sign upon the other, let us suppose b equal to unity, and we shall find, according to the foregoing principles, $-a \times -b = +a$; then taking any other value of b, as 10, the result will be $-a \times -b = +10a = +ab$. To show that the result of $+a \times +b$ must be similar to that of $-a \times -b$, it may be observed that as a new result of the multiplication $+a \times +b$ is produced by the alteration of *one* of the signs, as $-a \times +b$, so the original result will again be equally and indifferently brought about either by the restoration of the original sign belonging to a, when it will stand, as before, $+a \times +b$; *or* by the further additional reversal of the sign of the *remaining* term $(+b,)$ when the *same* product will be represented by $-a \times -b$: hence $+a \times +b = -a \times -b$; and hence a *negative* quantity multiplied into a *negative* quantity gives a *positive* result.

Trusting you will excuse a degree of prolixity, and even of tautology, which appeared necessary to elucidate a subject extremely exposed to difficulty and misconstruction,

I remain, Sir,

Your very obedient servant,

Paddington Green, Feb. 20, 1815. JOHN DILLON.

XXVI. *Notices respecting New Books.*

A Treatise on the Construction of Maps; in which the Projections of the Sphere are demonstrated, and their various practical Relations to mathematical Geography deduced and explained, systematically arranged, and scientifically illustrated from Twenty-eight Plates of Diagrams; with an Appendix and copious Notes. By Alex. Jamieson. 8vo. pp. 202.

THE claims of the author are modest: "In a science that has outlived the vicissitudes of two thousand years, and become splendid amidst even the riot of barbarism, originality is hardly to

to be expected. If, however, the compiler has successfully arranged and condensed the scattered researches of various authors, and united the theory of ancient with the practice of modern discoveries, he has perhaps done all that could reasonably be expected." It is but justice to say, that the author has done all this, and done it well. The work is divided into ten sections. The 1st contains preliminary remarks on the nature of the terrestrial globe, its circles, axis, poles; on latitude, longitude, and the positions of the sphere: the 2d presents preliminary observations on maps and charts. These serve very naturally to introduce the student to the 3d section, in which "the principles of the orthographic, the stereographic, and the globular projections of the sphere are fully demonstrated; and the last of these is investigated in a manner entirely new, to prove its superiority and admirable fitness in the construction of maps.

"In the 4th section theory descends to practice; and, as certain combinations are proposed to be effected, the projections are handled in the form of problems." The 5th section is devoted to Mercator's projection; the origin and properties of the rhumb line occupy the 6th; the 7th embraces the meridional, equatorial, and horizontal construction of maps. The principles of developing a spheric surface on a plane are investigated in the 8th section. In the 9th, which unfolds numerous projections of particular maps, the constructions are presented in a popular form, and include every thing of essential service in modern practice. In the 10th section is shown the extensive application of the orthographic projection of the sphere in the construction and use of the Analemma—an instrument which, with the assistance of good maps, will not only solve many curious problems, but be almost equivalent to a terrestrial globe.

The Appendix contains various methods of drawing large circles and ellipses; directions for colouring maps; a catalogue of some of the best maps, and general rules and observations for judging of the accuracy of maps. In the notes and illustrations with which the work concludes, various useful historical and critical remarks occur, drawn from numerous sources, some of which are difficult of access.

History of the University and Colleges of Cambridge.
By G. Dyer, *A.B.* Two Volumes, 8vo.

We have perused these volumes with much pleasure; and have no hesitation in saying they are calculated to afford not only amusement, but much useful information and instruction. The general subject is not of a nature to demand much room in a work like ours; but the 8th and 9th chapters being short, and falling within our plan, we lay them before our readers.

Chap.

Chap. VIII.

Age of Science—Philosophy—Bacon, and others.

" We must now speak of Science: for the period from Erasmus to Bacon may be called the age of literature; that from Bacon to Newton, of science and philosophy. And Cambridge feels a pride in ranking both Bacon and Newton among her sons.

" Bacon has been called the restorer of science, by raising it on a broader basis, and, in contradistinction to the ages which preceded him in this country, by deducing it from experience and observation, rather than uncertain rules and precarious ratiocinations. He was of Trinity College. He, at length, rose to be lord chancellor of England: but, from his childhood, was so generally conversant in books, and of such comprehensive intellect, that queen Elizabeth, to whom he was known through his father, Sir Nicholas Bacon, used to call him, when but seven years of age, her young lord keeper.

" Sir Nicholas is well described by one, who had well studied the characters of queen Elizabeth's ministers, as " an arch piece of wit and wisdom; as a gentleman and a man of law; and of great knowledge therein, whereby, together with his other parts of dexterity and learning, he was promoted to be keeper of the great seal:" such, too, was the literary character, and such the political progress of the son*; and both were of Cambridge.

" Lord Bacon's aim was to point the readiest way to universal knowledge; to shew how what the ancients had done might be rendered more perfect, and the human mind directed to new discoveries†. With these views he published, in 1605, his two books on the Advancement of Learning, and dedicated them to James I. But the aim of this extraordinary performance‡ is best described in his own language. " I have taken," says he, in a letter to lord treasurer Burleigh, " all knowledge for my province; and if I purge it of two sorts of rovers, whereof, the one with frivolous disputations, confutations, and verbosities, the other with blind experiments, and auricular traditions and impostures, hath committed so many spoils, I hope I should bring in industrious observations, grounded conclusions, and profitable inventions and discoveries, the best state of that province." A few years afterwards he sent these letters to Dr. Playfair, Lady Margaret's professor, to be translated into Latin: but herein he

* Observations on the Life of Sir Nicholas Bacon and Sir Francis Bacon, in Lloyd's Statesmen and Favourites of England. P. 287 and 600.

† Visum est enim nobis, etiam in iis quæ recepta sunt, nonnullam facere moram; eo nimirum consilio, ut facilius veteribus perfectio, et novis aditus, detur. Singularum Argumenta ad Augm. Scient.

‡ A neat little edition of this work was published in 1808, by Mr. Mallet, formerly of Trinity College.

was

was not so successful, at first, as King James: Bacon not liking the specimen returned him of Playfair's Latinity. They were, however, translated afterwards by other hands.

" In the year 1629, he published his *Instauratio Magna*, called Novum Organon, a title taken from his great predecessor Aristotle*, to the materials of whose writings he was greatly indebted, though he raised them on his new foundation, as Locke was greatly indebted to Hobbes's foundation, though he shaped his materials into a different form. Bacon, in a letter to the king, says of his Novum Organon, 'I hear my former book, of the Advancement of Learning, is well treated in the Universities here, and the English colleges abroad, and this is the same argument and deeper.'

" In 1623, he published his famous work, de Augmentis Scientiarum, which, however, can scarcely be called a new book, it being an enlargement of his Advancement of Learning, put into a Latin dress; in the adjusting of which George Herbert, the Cambridge poet, and Hobbes, the philosopher of Malmesbury, gave their assistance: which reminds me, that Bacon's practice was much imitated by Hobbes, some of whose after-works were but enlargements of former experiments.

The

* Aristotle, in the opinion of every one, was a most extraordinary man; and wrote upon all subjects, metaphysics, physics, mathematics, mechanic questions, physiognomy, morals, politics, and poetry, &c. Diogenes Laertius de Vitis, &c. Philosophorum, lib. v. p. 323, edit. Casauboni, 1584, says, there were 40 volumes of his, of the authenticity of which there was no doubt: Α τον Αριθμον εγγυς ηκει τετρακοσιων, τα οσα γε αναμφιλεκτα. Many are lost, of which one was probably the most valuable of any, πολιτειαι πολεων δυοιν εξηκοντα και εκατον; The Governments of 162 cities. Some Fragments of this work were collected from ancient writers by Isaac Casaubon, and published.

Of Metaphysics (μετα των φυσικων), as he terms them, he has treated at large. Of Grammar and Logic, it does not appear that Aristotle treated, as they have since been formed into systems or arts, and taught in the schools. The works of Aristotle, read by the *Scholastics*, at Cambridge, were Latin Translations by the Arabians, incorrectly made, and often crudely wrought into their own theories.

At, and since the revival of letters, various arts of Logic were published by Ramus, Crellius, Bertius, Hunnæus, Molineus, and Keckerman, and after them by Bergersdicius. Of these, some profess to follow Aristotle; others, to hold him in contempt: but as Aristotle has not treated distinctly of *method*, the 4th instrument in Logic, and as, probably, the Categories, or Predicaments, were not his; and as he never formed his Treatises into a system, or Art of Logic, we may be often led into mistakes about Aristotle. I say the Categories were probably not Aristotle's, because Diogenes Laertius mentions only one book of Categories, Κατηγοριων; ed. Diog. Laertius, ut sup. p. 317: whereas, the Categories as we now have them, consist of three parts, regularly divided into chapters. Aristot. Op. omnia, vol. i. edit. Du Vall.

Bacon's Novum Organon may be considered as levelled against all those several arts; but more particularly against the Analytics and Topics of Aristotle, which treat so largely of Syllogisms. It should, however, be observed, that though we are in the habit of speaking of INDUCTION, as Bacon's, that Aristotle, also, has Induction; Και τροπον τινα αντικειται η επαγωγη τω συλλογισμω· ο μεν γαρ δια τȣ μεσȣ το ακρον τω τριτω δεικνυσιν· η δια δε τȣ τριτου το ακρον τω μεσω. Analytica priora, lib. ii. cap. 23.

"The literary character of Bacon, and the uses to which his writings have been put at Cambridge, (for his principles have been most successfully followed there,) are admirably expressed by an elegant genius of our sister university: 'One of the most extensive and improved geniuses we have had instance of in our own nation, or in any other, was that of Sir Francis Bacon, Lord Verulam. This great man, by an extraordinary force of nature, compass of thought, and indefatigable study, had amassed to himself such stores of knowledge as we cannot look upon without amazement. His capacity seemed to have grasped all that was revealed in books before his time; and not satisfied with that, he began to strike out new tracks of science, too many to be travelled over by any one man in the compass of the longest life. These, therefore, he could only mark down, like imperfect coastings on maps, or supposed points of lands, to be further discovered and ascertained by the industry of after ages, who should proceed upon his notices or conjectures*.'

"Bacon's new philosophy deranged the old, which, in truth, at least as it had been long taught in the schools, was putting the cart before the horse, and has occasioned Horn Tooke to say, justly, in the sense he there uses the term, 'If they give up their doctrine of language, they will not be able to make a battle for their *metaphysics*, the very term *metaphysics* being nonsense; and all the systems of it, and controversies concerning it, that are, or have been in the world, being founded in the grossest ignorance of words, and of the nature of speech†.'

"Bacon's new philosophy, then, aspired to derange the old metaphysics and logic, and with them the old natural philosophy, the subtleties of the former being the foundation of the latter. It, however, left a space open for a more liberal philosophy, founded in the operations of nature, and uniform experience. As far as logic and metaphysics went, that place was filled up by Locke's Essay on the Human Understanding;—his inquiry being, in fact, a guide to *general* metaphysical reasoning, a philosophical analysis of the principles of logic (as some part is of grammar), and founded on the principles of Bacon, as the more sure method of philosophizing‡.

"Locke's

* Addison. † Diversions of Purley, vol. i. p. 399, 2d edit.

‡ Bacon gives this account of Syllogism: "Syllogismus ad Principia Scientiarum non adhibetur: ad media Axiomata frustra adhibetur, cum sit subtilitati naturæ longe impar:" and, again, "Syllogismus ex propositionibus constat, propositiones ex verbis, verba notionum Tesseræ sunt. Itaq. si notiones ipsæ (id quod basis rei est) confusæ sint, et temere a rebus abstractæ, nihil in iis quæ superstruuntur, est firmitudinis. Itaq. Spes est una in Inductione Vera.

Novum Organon, lib. i. 13, 14.

Of the two ways of reasoning by Syllogism and Induction, he elsewhere observes, "At in formâ ipsâ quoque Inductionis, et judicio quod per eam fit, opus longe

"Locke's book being expelled from Oxford, found a more ample reception at Cambridge: for, though a Fellow of Emmanuel College ventured to write a sturdy volume* against his Essay, it soon became a text book in the University, and the ablest metaphysicians were proud to be its critics and commentators. Hartley† was a disciple of Locke's school: his doctrines of the Mechanism of the Human Mind, and of the Association of Ideas, are but an enlargement of Locke's, or rather a deduction from it. His Doctrine of Vibrations is considered more his own‡; and though Hartley's *Observations* has not been made a Lecture-book in our colleges, it has been much read in the University. Dr. Law, late bishop of Carlisle§, published in 1777 a fine edition of all Locke's Works, together with a Life and Preface; and the Moral Philosophy of Dr. Paley is fruit of the same tree, though damaged in the gathering.

"As Logic (and with it Metaphysics) had been so greatly taught at Cambridge, prior to the revival of letters, it may be proper to observe, that the Scholastics considered thought as making no use of a bodily organ, and, indeeed, as having no communication with the body. They conceived the soul to be the *place of ideas*, τοπος των ιδεων, and logic, like experimental philosophy, as having *instruments* corresponding to the third operation of the mind, judgement, and with them comparing together those ideas, and making inferences by the assistance of Syllogisms.

"The art therefore was supposed by Bacon and Locke to have been exercised in the schools with too many subtleties and fleeting uncertainties, deduced from Aristotle, who, making it

longe maximum movemus. Ea enim de qua Dialectici loquuntur, quæ procedit per enumerationem simplicem, puerile quiddam est, et precario concludit, et periculo ab instantia contradictoria exponitur, et consueta tantum intuetur, nec exitum reperit. Atqui opus est ad Scientias Inductionis forma tali, quæ experientiam solvat, et separet, et per exclusiones ac rejectiones debitas necessario concludat. Quod si judicium illud vulgatum Dialectorum tam operosum fuerit, et tanta ingenia exercuerit; quanto magis laborandum est in hoc altero, quod non tantum ex Mentis penetralibus, sed etiam ex Naturæ visceribus extrahetur? Bacon, accordingly, gives up Syllogism.

* Anti-Scepticism, or Notes upon each Chapter of Locke's Essay, concerning Human Understanding, in four Books. By Henry Lee, B. D., formerly Fell. of Emmanuel College. 1702.

† Observations on Man. Mr. Hartley was of Jesus College.

‡ So far only as the English school goes. It was taught by the French philosophers. See Systême de la Nature, par M. Mirabaud, part. prem. chap. 8, 9. "Cependant, si nous voulons nous en faire une idée précise, nous trouverons que sentir est cette façon particulière d'être remue, propre à certains organes des corps animés, occasionnées par la présence d'un objet matériel qui agit sur ses organes, dont les mouvemens ou les ébranlemens se transmettent au cerveau.—Dans l'homme, les nerfs viennent se reunir & se perdre dans le cerveau."—In my Poetics, however, I have stated an objection to the doctrine of Vibrations.

§ He was Master of Peter House.

consist

consist too much in artificial rules, fell short of the philosophy of the art. Such, indeed, was the authority of this philosopher (an extraordinary man after all), that in logic and metaphysics he for many years entirely swayed the English and Scottish universities, as he did those of all Europe, till they came to the chapter of Ethics, where, having, according to the theology of most of the reformed churches, embraced the doctrine of *Original Sin,* they abandoned the Aristotelian for another standard*. Then Bacon came; and with his powerful thoughts, varied reading, and brilliant compositions—and never perhaps in the same man was there a more extraordinary combination, so rich and multiform—and gave irresistible weight to the cause; a philosopher, often too much of a politician, and, in his turn, not to be uniformly admired, nor explicitly followed.

"But though Bacon's and Locke's writings proceed upon a larger scale, for the purpose of philosophical inquiry, logic is still incidentally taught in our colleges, and syllogism still followed in our schools. Various books were published on the subject at Oxford; and Burgersdicius was republished at the university press of Cambridge†. But perhaps the best system of logic, or, at least, that most favourably now received at Cambridge, is little more than an abridgment of Locke's Essay‡. This must suffice for logic and metaphysics.

"But, we must not close this article without noticing the metaphysico-mathematical Demonstration of the Being and Attributes of God, by Dr. Samuel Clarke. It has obtained many admirers at Cambridge, as a subtle, elaborate performance; but the arguments of it were far from being approved by the metaphysicians of his age, and were probably not satisfactory to himself. We should remark, that Spinoza and Mirabaud § have employed the same arguments to prove the eternal existence, &c. of matter, which Doctor Clarke applies to spirit; and, in short, Dr. Law has shown, that the subject is not ca-

* Aristoteles primum quasi fundamentum felicitatis, virtutis, deliberationis bonæ, et electionis, constituit rationem humanam, per se puram, integram, et incorruptam.

Nos itaq. quibus ex agnitâ veritate revelatum est, hominum bene intelligendi, volendi, deliberandi, et agendi facultatem a lapsu primævo penitus destitutam esse, ab Aristotelis sententiâ de felicitatis, virtutum et bonarum actionum fundamento recedere cogimur.—Thesis Philosophica, 1599.

† Fr. Burgersdicii Institutionum Logicarum Libri duo were first published at Leyden two or three years after Lord Bacon's Novum Organon, and reprinted at Cambridge in 1668.

‡ Duncan's Logic.

§ Descartes, Pascal, le Docteur Clarke lui-même, ont été accusé d'Atheisme, par les Théologiens de leurs tems; ce qui n'empêche point que *les théologiens subséquens ne fassent usage* de leur preuves, et ne les donnent comme très valables.—Système de la Nature, part. sec. chap. 4.

 pable

pable of Dr. Clarke's sort of reasoning, from cause to effect (the *a priori* argument, as it is called), but only from effect to cause (the *a posteriori* argument). However, religion, which consists in the veneration of the incomprehensible Being, God, that made us, and is more a matter of feeling than of mathematical reasoning, or metaphysical distinction (as made in the schools), is little concerned in the dispute.

" A word or two on grammar. But as we have had occasion to speak of a philosophical logic, higher than the mere vulgar art of scholastics, so here, in contradistinction to the grammar of the schools, I must be understood to mean Philosophical Grammar. Nor will the reader be surprised to hear it spoken of in a history of university literature, if he recollects, that not only in our ancient monasteries, and colleges derived from them, there were regular grammar schools*, but that as late as the time of archbishop Parker, students graduated in grammar distinct from arts†, as well as in theology, or canon and civil law.

" There has been an attempt at Philosophical Grammar, written by one duly related to our Alma Mater, and it is in immediate reference to our own language: the attempt has succeeded. The object of the Diversions of Purley (as the book is entitled) is explained by the author himself in a few words: speaking of his abbreviations, or winged words, he says, " I imagine it is in some measure with this vehicle of our thoughts, as with the vehicles of our bodies. Necessity produced both. Abbreviations are the vehicles of language, the wings of Mercury." In contradistinction, therefore, to the eight parts of speech, as taught in our ordinary grammars, Mr. Tooke maintains, that, strictly and philosophically speaking, there are only two, the noun and verb: the remaining parts of speech he considers merely as abbreviations of these.

" The author of The Diversions of Purley lays claim to the notice of his Alma Mater, by the following singular address, prefixed to his book—" To the University of Cambridge, one of her grateful sons, who always considers acts of voluntary justice towards himself as favours, dedicates this humble offering‡."

" It is true—The Diversions of Purley is not a lecture-book, any more than many other works already mentioned. Being, however, recommended by a plea of such filial feelings, and being itself a powerful performance, a *characteristic* work, it claims a place in a History of Cambridge literature.

" We have thus traced our Alma Mater through her different stages of literary progress; through her dark or obscure age, her scholastic age, her intermediate, or, as it is called in the Ap-

* Monastικῶν, lib. 1. † Acad. Hist. Cant. p. 47.

‡ Επεα Πτερόεντα, or The Diversions of Purley, 2d vol. 1798.

pendix

pendix to Dr. Cave's Historia Literaria, the Wickliffian age (in which new doctrines were contending for superiority with old), and through her literary or reformed age, commencing with the revival of literature, at the Reformation: we must follow her now to her last, that is, her mathematical age."

Chap. IX.

Mathematics—Dr. Barrow, Sir Isaac Newton, Mr. Whiston, and others.

"Mathematics, by the ancients, was called by eminence *the learning*, and diva mathesis, the *divine mathematics;* yet not till a late period did Cambridge University cultivate it, with much devotion or success*: but having, at length, found the true avenue to its temple, they have approached to its most intimate recesses.

"Though there were doubtless (before the time of Dr. Barrow) men of much mathematical knowledge at Cambridge (of whom notice will incidentally be taken in the proper place), yet the mathematical age properly commences with him; his Prælectiones Mathematicæ being the book that preceded in course of due mathematical investigation. He was born in 1630, and was appointed Master of Trinity College by Charles II. Other eminent mathematicians were nearly contemporary with him, such as Dr. Smith, and Mr. Cotes, of Trinity College, and Mr. Whiston, of Clare Hall, and others. But they may all be considered as the precursors, or the genuine successors of Sir Isaac Newton. Newton was of Trinity College, was born at Woolsthorpe, in Lincolnshire, in 1642, and lived to a good old age, though all his discoveries were made and completed in the earlier period of his life. He died in 1727.

"His great work, *Naturalis Philosophiæ Principia Mathematica*, was first printed in 1687. It was the same light which beamed on Bacon, which guided Newton to his discoveries: what the former considered as desiderata, the latter supplied. Prior to their time, the mode of philosophizing consisted in assigning to each species of things their specific and occult qualities, from which all the operations of bodies, by some unknown, mysterious order, proceeded: this was the philosophy of the Peripatetics;

* ——— Pudet hæc opprobria nobis
Et dici potuisse, et non potuisse refelli.

Whiston goes on: Et pudeat nobis non immerito; his præsertim temporibus, quibus scientiæ mathematicæ florent alias ubiq., et excoluntur; quibusq. veram Physicam a Mathesi dependere unice, sit adeo certum et exploratum. Quinimo illud vel maxime fuit opprobrio, quod jam tum mathemata nobis academicis minimæ fuerunt curæ, cum Ducem et Professorem ipsum Newtonum, Geometrarum hujus Ævi, ne quid amplius jam dicam, facilè Principem, habuerimus. Prælec. Astron. Hab. Cantab. Anteloquium. 1707.

patetics; and, having been implicitly adopted by the schoolmen, has been since called the Scholastic Philosophy: they affirmed that each effect of bodies flowed from its individual nature; but whence the several natures proceeded, they did not show; they were defective in observation and experiment, dwelling rather on the names of things, than on the things themselves. According, therefore, to the Newtonians, whose words I borrow, they had invented a philosophical language, but could not be said to have taught philosophy*.

"Some, indeed, according to the statement of the Newtonians, had, emerging somewhat from this obscurity of mere words, maintained that all matter was of the same kind; and that all the variety of forms, which we see in bodies, arises from the most simple affections of their component parts: but to those affections they assigned other modes than what, it appears, have been assigned to them by nature, indulging themselves in a liberty, which, however plausible to the imagination, was not founded in reality; they conceived certain unknown figures and magnitudes, positions and motions of parts, together with certain occult fluids, which, by entering the pores of the bodies, agitated them with great subtlety and force: here, too, it was insisted, they had no authority, from observation or experiment, their theory being all founded on conjecture: the Newtonians, on advancing these objections, had in view the doctrine of atoms and vortices of Descartes and his followers. The accuracy of many of their mechanical laws and deductions was admitted; but their speculations were considered as mere hypotheses, 'fabulam (said the Newtonians) elegantem forte et venustam, fabulam tamen concinnare dicendum esse.'

"Thus was Newton led on to that third way of philosophizing, called experimental: he assumed no principle that was not sanctioned by phænomena, and from the most simple principles he aimed to arrive at general causes, and original laws: hypotheses he laid down not as systems to be believed, but as questions to be tried; and he proceeded by a twofold method, which he called analytic, and synthetic: he deduced the more simple powers and laws of forces from certain select phænomena; this he called *analysis;* and then proceeding from those single phænomena to more general and comprehensive forms, he established *synthesis*. Not that this way of proceeding by analysis and synthesis are novelties, they are noticed by Aristotle. But these are the rules followed by Sir Isaac Newton, in his way of philosophizing, and by these he established a theory, which was said to explain and illustrate the system of the universe.

"According to this theory, then, it was maintained, that all bodies

* Newtoni Principia Philosophiæ Naturalis. Pref. a R. Cotes.

bodies had a tendency to gravitate, mutually, and to some centre; that they had a twofold force, one urging them to move forward, in a straight line, the other downward, to a centre; the two forces combined forming a curve: thus he accounted for the motion of the heavenly bodies, and all proceeded on mathematical demonstrations, accompanied with the calculations of algebra.

"The other branches of philosophy, as well as astronomy, were, in like manner, brought to the test of experience, and subjected to mathematics and algebra.

"Such was the philosophy of Newton.

"The science which had been laid down by this great man in the profoundest speculations, was opened in a more popular manner by others, particularly by Whiston, as already observed, in the public schools.—What has been said above upon this subject, has been delivered nearly in the Newtonian's own words; and it is not necessary to proceed further: suffice it that this philosophy, made up, as we have said, of mathematics and algebra, constitutes now the principal discipline, and prime glory, of the University of Cambridge."

Mr. Parkes, the author of "The Chemical Catechism," has now in the press a Series of Chemical Essays, which he designs to publish in four pocket volumes, including a variety of explanatory notes and a copious index. These Essays are written in a familiar style, so as to suit those readers who are not yet proficient in chemical science, and they embrace an assemblage of curious and interesting subjects in the economy of nature, as well as on some of the most important manufactures of this country. The work will be illustrated with more than twenty copperplate engravings, and all from original drawings, either of new chemical apparatus, or of such improved machinery as is now employed in the respective manufactures on which the author has treated in these Essays. The whole is in considerable forwardness, and will probably be ready for delivery by the end of March or beginning of April.

Mr. Accum has in the press "A Treatise on Gas Light," exhibiting a summary description of the apparatus and machinery best calculated for illuminating houses, streets, and public edifices, with carburetted hydrogen or coal gas; together with remarks on the utility, safety, and general nature of this new branch of civil economy. This treatise will be illustrated with geometrical and perspective designs, exhibiting the larger gas light apparatus, now successfully employed for lighting the streets

and houses of this metropolis, as well as the smaller apparatus used by manufacturers and private individuals, and other objects connected with this new art of procuring artificial light.

XXVII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Feb. 2. The right hon. President in the chair. The conclusion of Mr. Cooke's paper on Nautical Charts was read. The paper was divided into three parts, accompanied with diagrams and mathematical demonstrations: the author dwelt particularly on a mode of making graduated circles or charts, so that angles might be taken on the charts simply by means of a common rule, and also sextant observations at any place. It does not appear that the author is acquainted with the graduated circle in use among the French mariners, but which is not much esteemed by the English.

Part of a long elaborate and curious paper by Dr. Wilson Phillip of Gloucester was read, detailing a series of experiments made to ascertain the relative influence of the nervous system on the functions of the heart. The ultimate object of these inquiries was to determine the effects of poisons, and their manner of destroying animal life: the present experiments were only preliminary.

Feb. 9. Dr. Phillip related the result of twenty-four experiments made on warm and cold-blooded animals by various modes of removing or destroying the brain, or cutting off the communication between it and the heart or spinal marrow. The ten first experiments were on rabbits, and the remainder on frogs. He found, contrary to the statement of M. Le Gallois, that the circulation of the heart was not arrested by blows on the occiput so as to crush the brain without greatly injuring the spinal marrow; that the heart retained its irritability for twelve hours after the nerves of the head had been completely detached from it; and that the spinal marrow has some influence on the heart. The author next tried the effects of opium and other stimuli on the spinal marrow, &c. of a frog, and found that the muscles of the heart of frogs could be stimulated till irritability was completely exhausted. He concludes, that there are three distinct powers in the animal body; the sensorial, which exists in the cerebrum; the nervous properly so called, which belongs to the spinal marrow; and the muscular power, which exists in the muscles; and that these powers exercise a mutual influence on each other. With M. Le Gallois, he thinks that the spinal

marrow is acted on by the brain in the same manner as the muscles of the heart are acted on by the spinal marrow. Nevertheless, although the heart retains its contractibility after the nerves of the brain are removed, he admits that violent joy will destroy its functions quicker than decapitation. Respiration appears to depend on the nervous system; the heart is capable of contraction like all other muscles; the involuntary muscles obey the same law as those of voluntary motion.

Feb. 16. Mr. Cliff, in a letter to Sir E. Home, described the effects of several experiments made to ascertain the influence of the spinal marrow in fishes, particularly carp. The hearts of two carps were laid open, and the fish immersed in water: in an hour the fins ceased to move, but the gills continued to act; in two hours the latter became motionless, and in three hours and forty minutes all action of the heart had entirely subsided. In another experiment, the heart was laid open and exposed to the air, when its pulsations were from twelve to twenty in a minute. A red hot wire was passed up the spinal marrow to the brain of a carp: still the action of the auricle and pulsation continued for six hours and a half. It appeared that the heart's action when laid open and exposed to water is sooner destroyed than when left in the air: exposing the heart accelerates its action or quickens the pulsation for a time, which gradually subsides. In these experiments the removal of the brain produced no immediate effect on the action of the heart.

A letter from Dr. Brewster to the President was read, describing some of this philosopher's experiments on a new property of light from the second surface of reflecting bodies.

Feb. 23. Part of an erudite paper by Sir Humphry Davy was read, On the Composition of the Paints used by the Greeks. Sir H. in the introduction took a review of the progress of painting among the Greeks, a people who had an innate taste for the beautiful and the magnificent; he next traced the march of the arts from Greece to Rome, and lastly proceeded to an analysis of the colouring matter of the remains of the Greek paintings found on the walls of Herculaneum and Pompeii. The principal colour in these paintings, it appears, consisted of carbonates of copper prepared and blended in different proportions.

ROYAL INSTITUTION.

Mr. Brande has commenced a course of lectures on the rise and progress of chemical philosophy, and its applications to the arts, in which he proposes to unfold the gradual advance of the science, and to illustrate it by experiment.

The first lecture embraced a view of the early periods of chemistry,

mistry, in which, after briefly noticing the chemical records of ancient nations, Mr. B. proceeded to the age of Alchemy, the chief origin of which he referred to the new Platonists, whose rise marked the declining age of learning towards the end of the third century.—The mysteries of transmutation and the season for the elixir of life were so well adapted to the genius of professors of dæmonology and magic, as to acquire among them rapid celebrity; and the history of their wanderings is not unimportant, as connected with the subsequent advances of chemistry. The best specimen of alchemical jargon is to be found in works written by or attributed to Geber, who is supposed to have been an Arabian prince of the seventh century. Dr. Johnson derives the word gibberish from the language of Geber and his tribe.

As characteristic writers of the thirteenth and three succeeding centuries, Mr. B. selected Roger Bacon, of Somersetshire; Basil Valentine, of Erfurt; Paracelsus, of Switzerland; and Van Helmont, of Brussels.

Entering upon the seventeenth century, the Professor furnished a sketch of the life and writings of Lord Bacon, and of the new turn which he gave to experimental philosophy. It was not till then that science shook off its deformed and sickly aspect, and acquired new and healthful vigour. Glauber and the Honourable Robert Boyle were cited as leading authors of the seventeenth century: the writings of the former are full of keen observations: the claims of the latter to originality were not great; but the benefits which his patronage conferred upon science were numerous and lasting. He brought Hooke into notice, and put him in the road to eminence.

After a rapid glance at the discovery of the air pump and barometer, Mr. Brande noticed the foundation of learned societies. The Academy del Cimento was established in 1651 by the Duke of Tuscany; the Royal Society in 1662; and the Royal Academy of Sciences in 1666. Charles the Second, though not quite indifferent to the welfare of science, conferred but empty honours on the Royal Society, but Louis the Fourteenth was more generous to the Parisian establishment. This liberality is not only highly honourable to his memory, but in the eyes of the ingenious part of mankind has ever been regarded as no insignificant atonement for the multitudinous errors of his reign.—Happily for the scientific character of Britain, said the Professor, the genius, talents, and exertions of less exalted individuals have ever counterbalanced such advantages; and thus protected, the growth of science has been as vigorous, and perhaps more healthy than in the sunshine of royal favour.

The lecture concluded with an eulogium on the chemical merits of Newton; with experimental illustrations of his views; and

and a recapitulation of the prominent events of the period. When alchemy was at its acme, during the fifteenth and sixteenth centuries, many useful members of society were entrapped by its golden prospects. Among them was that admirable artist, Mazzuoli of Parma, better known under the name of Parmegiano: sickness and beggary were the reward of him and his associates.

The zeal and activity in scientific pursuits, which has since marked its progress in Europe, became manifest early in the seventeenth century, and the causes which Mr. Brande unfolded, contributed to the splendour which it began to acquire, about the end of that important æra, in the general history of the world.

Mr. Brande's second lecture embraced an account of the state of chemistry at the opening of the eighteenth century, in which the theories of combustion invented by Beccher, Stahl, Rey, and Mayow, were principally dwelt upon, and experimentally illustrated. The system of Beccher, as enlarged and embellished by his pupil Stahl, supposes inflammable bodies to contain a peculiar volatile principle called phlogiston; flame is produced by the escape of this principle. Rey in 1630, and Mayow in 1674, objected to the conclusions upon which the phlogistic system was reared; for bodies in general, they observed, became heavier by combustion. Tin, lead, and other metals, burn when heated to redness; and the residuum, or result of combustion, is heavier than the metal they set out with. Combustion, therefore, said they, consists not in the separation of phlogiston, but in the fixation of air, and substances refuse to burn when air is excluded.

Mr. Brande illustrated these positions by many of the original and curious experiments detailed in Mayow's treatise on the nitro-uric spirit. In speaking of Rey's publication, the Professor observed that its present scarcity was enigmatical, it having been reprinted at Paris in 1777; but it contained many doctrines to which the new French theorists have laid claim, as he should prove in detailing the history of the antiphlogistic theory, when the neglected merits of Rey and Mayow would again call for attention.

Having paid a just tribute of praise to the labours of Hales and Boerhaave, the founders of pneumatic chemistry, Mr. Brande gave an account of the invention of the thermometer, an instrument which tended materially to the progress of that difficult and refined branch of chemistry, relating to the nature and properties of heat. Santorio was the first who constructed the air thermometer, which was much improved by Van Helmont. The Florentine Academicians used smaller tubes filled with spirits of wine; the instrument was perfected by the dexterous ingenuity of Fahrenheit, a bankrupt merchant of Dantzic.

The

The lecture concluded with some observations upon the anomalous expansion of water under the 42d degree of Fahrenheit's scale, and its importance in retarding congelation.

"Chemistry," said Mr. Brande, "was tardy in its progress, until the period at which he had now arrived in its history; in the dross of the alchemical furnaces many scattered treasures were discovered, of which the value was greatly enhanced by arrangement and systematic combination: the science thus acquired a prosperous aspect—it was applied to the arts, and to them it gave an unexpected and vigorous impulse; it was directed to the investigation of nature, and there it displayed new beauties—it found

> 'Tongues in trees, books in the running brooks,
> Sermons in stones, and good in every thing.'"

ROYAL MEDICAL SOCIETY, EDINBURGH.

This Society having agreed to appoint a Committee for the purpose of receiving the communications of members, and of others through their medium, who may favour the Society with interesting facts and experiments in medicine, or with unusual appearances in morbid dissection, beg leave earnestly to invite the members to transmit such communications to the Society as soon as possible.

The Committee, consisting of six extraordinary members resident in Edinburgh, together with the four Presidents, *ex officio*, will proceed immediately to consider such papers as may be transmitted to them with an ultimate view to publication.

KIRWANIAN SOCIETY OF DUBLIN.

Dec. 28. The reading of a paper was commenced, containing "A new Theory of Galvanism, with its Application to certain chemical Phænomena," by M. Donovan, Esq. Secretary.

The paper commenced with a review of the explanations of metallic arborizations offered by Bergman, Lavoisier, Vauquelin, Berthollet, Sylvester, and Grotthiuss. The theory of the two first-mentioned philosophers had been shown to be insufficient by Berthollet; and the view suggested by this last was conceived to apply well to some of the phænomena, but with much less effect to others which were adduced. The theories of Sylvester and Grotthiuss were stated to be the same in result: both suppose the commencement of the arborization to be chemical, and its progress electrical. It was observed, that the theory of the commencement lies under the objections which apply to Berthollet's view; and that both stages of the process should be referable

referable to the same cause. It was also considered improbable that the continuance of the arborization should be dependent on the decomposition of water.

Several arguments and experiments were then adduced to show that these phænomena depend upon laws of chemical affinity which have been hitherto overlooked. The affinity of one body for another will be modified, and even altered in a singular manner, by the contiguity of a third. This law of affinity was supported by a copious collection of experiments. Arborizations in several of their varieties and circumstances were investigated, and referred for explanation to the general inferences from the preceding facts. The result of a great number of experiments proved that the order of the changes produced on the affinities of bodies by the proximity of a third, exactly corresponds with the order in which metals precipitate each other in the metallic state from their solutions.

Jan. 11. The reading was continued. Some considerations were adduced to show that affinity is a force existing between every species of matter; and even between those that apparently exert no action on each other. The principle before stated, namely, that the attraction of one body for another will be peculiarly modified by the contiguity of a third, was referred to; and it was shown that this change is reciprocal, the third body suffering a change of another nature by the contiguity of the first. Considerations were then offered, from which it was deemed necessary to admit a division of bodies into two grand classes according to their species of affinity. The foregoing principles with some others of less consequence were supposed capable of explaining not only the internal action of the galvanic series, but also the effects produced by galvanism on other bodies. It was stated, that for reasons elsewhere assigned* the author conceived galvanic phænomena to be unconnected with electricity any further than that the latter is a concomitant power; and it was supposed that every fact can be better explained without the agency of that power.

The various phænomena of decomposition were then referred for explanation to the new principles, and some were adduced which have never been hitherto satisfactorily accoumted for, and which were considered reconcilable to these views. Some new facts relating to the internal action of the galvanic series were also noticed, which militate with modern theories, but do not oppose that now proposed. The power of the series in producing electricity,

* In an essay presented by the Author to the Royal Irish Academy on the theories of galvanism, and their influence on the doctrines of chemistry, &c., which was honoured by that learned body with the prize.

effects

effects on the organs of sensation and motion, heat, light, &c. were then examined, and stated to be explicable according to the principles before referred to: and it was conceived that all these effects, as they occur, are irreconcilable to the electrical hypothesis.

The paper was illustrated by a variety of experiments performed before the society.

Feb. 8.—A paper "On the acidifying principle, accompanied with some observations on the theories of combustion," was read by J. O. Reardon, M. D.

XXVIII. *Intelligence and Miscellaneous Articles.*

ANNOUNCED METALLIZATION OF CHARCOAL.

To Mr. Tilloch.

Sir,—Mr. Dobereiner, Professor of Chemistry in the University of Jena in Saxony, informs me, that he has discovered charcoal to be *a metallic compound.* The following lines I have received from Mr. Dobereiner on this subject. F. A.

.... "I hope to be able to communicate to you the successful metallization of charcoal, which I have found contains a metallic base. In cast iron and in steel the metal is present in a metallic state, and may be separated from both of them by the united action of phosphorus and an alkali. Further particulars concerning this subject I will transmit to you in my next.

"*To Mr. Accum, London.* Dobereiner."

NATURAL HISTORY.

Chameleon, Colours, Bats, Parrots, Monkeys.

Mr. Forbes (in his Oriental Memoirs, an interesting and splendid work) when at Dazagan in Concan, then belonging to the Mahrattas, kept a chameleon for several weeks, and paid great attention to its changing colours. Its general colour was "a pleasant green" spotted with pale blue. Its customary changes were to a bright yellow, a dark olive, and a dull green; but when irritated, or when a dog approached, in which case fear was perhaps the operating cause, the body became considerably inflated, and the skin clouded like tortoiseshell, in shades of yellow, orange, green and black: in these circumstances it appeared to most advantage. The animal was most singularly affected by any thing black: the skirting-board of the room was black,

black, and the creature carefully avoided it; but if by chance he came near it, or if a black hat were placed in his way, he shrunk to a skeleton and became black as jet. It was evident, by the care he took to avoid those objects which occasioned this change, that it was painful to him. The colour seemed to operate like a poison.

"The fact," says the Quarterly Review, "is highly curious, and deserves further investigation. We know but little of the manner in which animals are affected by colours, and that little is only known popularly. The buffalo and the bull are enraged by scarlet, which, according to the blind man's notion, acts upon them like the sound of a trumpet. Is it because the viper has a like antipathy, that the viper-catchers present a red rag, when they provoke it to bite, to extract its fangs? Daffodils, or any bright yellow flowers, will decoy perch into a drum-net. He who wears a black hat in summer will have tenfold the number of flies upon it that his companion will have upon a white one. When more observations of this kind have been made and classified, they may lead to some consequences of practical utility. We have observed that black clothes attract and retain odours more sensibly than light ones:—Is it not possible that they may more readily contract and communicate infection? Speculations of this kind, when they occur to us, we scatter like seed by the way side. The old corpuscular philosophy has found an able advocate in Mr. Dalton, and in an age of careful and suspicious experimentalists it may produce useful results."

Obliged one night to take up his quarters in the tomb-chambers of a Mahomedan grave (for the houses were not to be defiled by admitting a Christian), Mr. Forbes had first to drive out the previous occupiers—some enormous bats, from their size denominated "flying foxes." These animals frequently measure six feet from wing to wing.

On the coast where Angengo is situated, the parrots are as much dreaded at the time of harvest, as a flight of locusts, or a desolating Mahratta army. They darken the air by their numbers, and when they alight in a rice-field carry off every grain in a few hours.

At Dhuboy, the capital of a district containing 84 villages, of which Mr. Forbes was appointed governor (after its surrender to General Goddard in 1780), he found as many monkeys as human beings. They seemed to have full possession of all the roofs of the houses; and they are sometimes called in as auxiliaries in disputes between neighbours. In quarrels of this kind they never have recourse to blows, but employ every kind of invective against each other and against all their relatives. The person who is worsted in this war of words, frequently takes an opportunity

opportunity to strew some rice over the roof of his adversary's house. The monkeys soon discover this treasure, and resort in crowds to pick it up: when finding that much of it has fallen between the tiles (which are not nailed down as with us), they make no scruple to remove them, and nearly unroof the house. When workmen cannot be procured to repair the roof, the rain is admitted, and ruins the furniture and the stock of grain.

Apothecaries' Hall, Feb. 20, 1815.

The Society of Apothecaries having completed several new arrangements in their laboratories, in which steam is employed for the purposes of evaporation, distillation, &c. such professional gentlemen as are desirous of viewing these improvements are informed, that they will be open for their inspection on the second Tuesday in March, April, May, and June, between the hours of two and three o'clock in the afternoon.

LECTURE.

Mr. T. J. Pettigrew, F.L.S. will commence his Spring Course of Lectures on Anatomy and Physiology, on Friday the 10th of March, at Half past Eight o'clock in the Evening precisely, at his House, No. 3, Bolt Court, Fleet Street, where Particulars may be obtained.

LIST OF PATENTS FOR NEW INVENTIONS.

To James Collier, of Grosvenor Street, West Pimlico, Middlesex, who in consequence of a communication made to him by the late Joseph Montgofre, a foreigner, hath become possessed of the method of making a machine or instrument intended to be denominated a "Creopyrite," by means of which power will be very œconomically obtained, and advantageously applied to the raisiug of water, and other useful purposes.—16th Jan. 1815. —6 months.

To Jean Frederick Marquis de Chabanus, of Thayer Street, Manchester Sq., for his method of extracting from fuel a greater quantity of caloric than hath hitherto been acquired, and applying it to the purpose of warming the room in which the operation is conducted, and also other rooms, by one single fire. 16th Jan.—6 months.

To Jean Rondoni, of Oxford Street, Middlesex, gent. who in consequence of a communication made to him by a certain foreigner, is in possession of certain improvements in the construction of dioptric telescopes.—20th Jan.—6 months.

To John Carpenter, of Truro, Cornwall, for a knapsack which prevents the wet coming between it and the back, and a pouch suspended from the shoulder-straps of said knapsack so as to counteract its weight.—20th Jan.—6 months.

METEORO-

METEOROLOGICAL TABLE

Extracted from the Register kept at Kinfauns Castle, N. Britain, Supposed Lat. 56° 23½′.—Above the Sea 129 feet.

1814.	Morning 8 o'clock. *Mean height of* Barom.	Morning 8 o'clock. *Mean height of* Ther.	Evening, 10 o'clock. *Mean height of* Barom.	Evening, 10 o'clock. *Mean height of* Ther.	Depth of Rain. Inch. 100	N° of Days. Rain or Snow.	N° of Days. Fair.
January.	29·78	24·64	29·80	25·39	·60	11	20
February.	30·06	34·03	30·04	34·50	·63	8	20
March.	29·90	36·48	29·91	36·80	·85	17	14
April.	29·90	46·43	29·90	46·10	1·75	18	12
May.	30·15	47·06	30·16	44·77	·75	8	23
June.	30·16	53·06	30·02	50·50	·57	6	24
July.	29·93	59·35	29·94	57·10	1·72	15	16
August.	29·91	56·77	29·93	54·86	2·60	18	13
September.	30·10	51·90	30·10	52·66	·57	8	22
October.	29·82	44,54	29·84	44·51	1·02	11	20
November.	29·74	38·70	29·74	38·16	2·30	12	18
December.	29·65	34·80	29·66	35·38	2·23	18	13
Average of the year.	29·925	43·980	29·920	43·394	15·59	150	215

ANNUAL RESULTS.

MORNING.

Barometer. Observations.	*Wind.*		*Thermometer.*	*Wind.*	
Highest, 11th May.	SE.	30·64	24th July,	SE.	64
Lowest, 16th Dec.	SE.	28·70	14th Jan.	W.	12

EVENING.

Highest, 10th May,	E.	30·65	25th July,	SE.	65
Lowest, 2d March,	SE.	28·91	15th Jan.	W.	12

Weather.	*Days.*	*Wind.*	*Times.*
Fair	215	N. and NE.	3
Rain or Snow	150	E. and SE.	109
		S. and SW.	65
	365	W. and NW.	188
			365

Extreme Cold and Heat, by Six's Thermometer.

Coldest, 14th January . . . . 7°
Hottest, 24th July . . . . 76°
Mean for 1814 . . . . 45·163

RESULT OF THREE RAIN GAGES.	In. 100
No. 1. on a conical detached hill above the level of the Sea 600 feet	33 8¼
— 2. Centre of Garden, 20 feet	20·05
— 3. Kinfauns Castle, 129 feet	15 59
Mean of the 3 Gages	23·16

METEORO-

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand,

For February 1815.

Days of Month.		Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Jan.	27	27	30	35	29·01	0	Snow
	28	35	41	40	28·99	5	Fair
	29	40	43	37	29·18	0	Cloudy
	30	37	40	36	·25	0	Cloudy
	31	36	44	42	·35	0	Cloudy
Feb.	1	44	47	40	·35	0	Rain
	2	39	44	39	·57	6	Cloudy
	3	39	45	40	·60	7	Cloudy
	4	46	50	40	·64	5	Cloudy
	5	40	48	44	·90	9	Fair
	6	44	47	44	·65	7	Cloudy
	7	45	49	43	·63	10	Fair
	8	44	49	42	·64	7	Cloudy
	9	42	47	40	·65	5	Cloudy
	10	40	44	43	·60	0	Small Rain
	11	44	47	41	·40	0	Small Rain
	12	45	48	40	·45	0	Stormy
	13	38	45	40	·51	12	Fair
	14	41	48	42	·58	11	Fair
	15	42	47	46	·65	5	Cloudy
	16	47	51	42	·52	0	Rain
	17	40	50	40	30·30	7	Cloudy
	18	42	50	46	·20	7	Cloudy
	19	44	49	46	29·99	6	Cloudy
	20	49	54	50	·60	0	Rain
	21	52	55	50	·95	8	Cloudy
	22	51	55	46	30·20	10	Cloudy
	23	42	50	46	·20	14	Fair
	24	46	51	47	.02	10	Cloudy

N.B. The Barometer's height is taken at one o'clock.

Erratum—In Mr. Gill's communication, for Mr. T. T. Hawkins read Mr. I. I. Hawkins.

XXIX. *Short Notices of Geological Observations made in the Summer of* 1814, *in the South of Yorkshire, and in North Wales, and of some Inferences therefrom, as to the Structure of England and Wales. By* John Farey, *Sen.*

To Mr. Tilloch.

Sir,—For several months past it has been my intention to send you during the present winter, an account, considerably in detail, of my Observations on the Strata during the last Summer, and of certain new Geological inferences which they seem to warrant, as far at least as the structure of England and Wales is concerned: but seeing reason now to fear, that Time will not permit the sending all the details intended, I must beg your insertion of a shorter Notice on the subject.

When I began, several years ago, to apply the *Smithian* principles of Stratification to practice, in the minute investigation of the Strata of particular Estates and their vicinities, and, with less minuteness, to much larger Districts, I fell in some instances into the error, to which observers of Nature have ever been liable, of concluding too soon, that I had become acquainted with all the leading characters of the Strata which I had investigated locally:—when for instance, I had traced my 1st, 2d, and 3rd Gritstone Rocks of Derbyshire, for more than 25 Miles, from Duffield northward, with but slight variations in their thicknesses or characters, as single Rocks, and had traced the Limestone-shale below and the 4th Grit, above these Rocks, still further northward, beyond Woodhead and Penistone, without material variations in their characters, I too hastily concluded (when about to finish my observations beyond Derbyshire on that side, in 1809) from observations at a few points, as to some other points, in the ranges of these three intermediate Rocks and their Coal-shales in this part of Yorkshire.

To some errors which were, in consequence hereof, printed in my Derbyshire Report, other more material ones having been added, by Persons who attempted to follow up the investigations I had begun, and disappointment following, in some trials that were made in search of Limestone, in Uden Vale, I was sent for last Summer, to complete my observations in this corner of Yorkshire, to the left of the old Road from Sheffield to Penistone. By this means I discovered the source of my former mistakes, in the new and unexpected characters which the 1st, 2d, and 3rd Rocks, and other intervening strata, exhibit, in these parts of their Ranges. Instead of the same entire thickness of these Rocks continuing, as above mentioned, each one was here found *much thicker*, separated into two distinct Rocks, by bind or

or soft and perishable stone, and making *two distinct Basset Ridges*, such, that had I happened in 1807, to have begun my Survey here, instead of near Ashover, these divided Rocks, must each have had separate names or numerical designations.

The positions which I at first adopted from Mr. Whitehurst (on reviewing nearly *the same local facts* as he had observed) as to *the only very coarse gritstone* of this series, occurring in the 1st Rock, and that "Coals are never found under coarse Gritstone," I in part corrected in 1809, by the discovery (first made near this border of Yorkshire) of the coarseness of the 3rd Rock, in numerous situations: but these now appeared, still further to be untenable positions, since in this corner of Yorkshire, the 2d Grit Rock was not only found *very coarse in some places* (and very fine and perfect, with concentric stains like shale-gritstone, in others), but the *Car-house Rock** in the 3rd Coal-shale, which in many places in Derbyshire appeared only as a very imperfect Stone or Bind, and even in a greater number of situations *could not be distinguished* (and therefore had not been numbered in my Series), here proved very coarse and thick, and formed the most conspicuous rocky Edges in the district! And, what will perhaps prove of greater interest to your Geological Readers, the 1st Coal-shale, not only produces here very coarse grit-stone beds, or large lenticular masses of such, and others that contain much calcareous matter, but it begins also to assume here, those metalliferous characters which have so long puzzled, in comparing the mining district of Yorkshire, Durham, and Northumberland, with that of Derbyshire; the Broomhead and Wigtwizle Mines of *Blende* and *Galena*, proving to be *in rake veins in the 1st Coal-shale!*

On examining the Coal-field of Anglesea Island 16 months ago, the appearances were so very dissimilar from the above, in the nearness of the workable seams of Coal to the Limestone, in the inconsiderable thickness of these calcareous Rocks, before coarse Slate commenced under them; leaving scarcely any space for the Toadstones, the Limestone-shale, or the 1st, 2d, or 3rd Grit Rocks and their Coal-shales, so that sufficient similarity did not remain, in these apparently extreme cases, to ground a conjecture, as to the identity of this Coal-field with that on the eastern side of Derbyshire. And the same appeared also to be nearly the case, with respect to the Denbighshire and Flintshire Limestone and Coal Fields, as far as my hasty journey across

* The situation and characters of this Rock, were fully described in the Spring of 1813, in my Paper and Maps and Section of the Ashover Denudation, which *had been promised a place* in the 2d volume of the Geological Transactions; but were subsequently denied *that honour* by a *Geognostic* faction, see vol. xlii. page 55, Note, and p. 217, Note.

them,

them, and a short stay, (in bad weather) with an ingenious Friend near St. Asaph, could enable me to judge: unfortunately, as it now appears, I had descended Rhualt Hill into the vale of Clwyd on its NE side in my way from Holywell, at a point where no Limestone was visible (owing, as it since appears, to its edge lieing low, covered by alluvial clay) and where nothing truly indicated *the direction of the dip* of the strata*: my Friends, whom I very fully questioned, were only able to tell me of three or four detached patches of Limestone, on all the north-eastern border of the vale of Clwyd, and which they believed to be perfectly *unconnected*, and *to dip north-eastwardly*, into the Mountain: from this, and what I had myself seen of the north-eastern dip of the two Limestone Rocks of Holywell (or Halkin) and Abergeley (or Cefn), I too hastily concluded, that there were in this part of North Wales *three* Limestone Rocks far separated by Slate, &c. agreeing nearly with those I had noticed near Ludlow, under-lieing the Clee-hills Coal-field.

During the last Summer and Autumn, opportunities have however been afforded me, by different Employers, of making a pretty minute Survey of all the Coal-field to the NW of Holywell, of examining other parts of it, and of satisfactorily tracing the Limestone underlieing the Coals of North Wales. Whereby I find, instead of there being three Limestone Rocks, widely separated by Slate, &c. that in general, four such Rocks occur, (as in Derbyshire, although not here separated by Toadstone) of variable thickness, and not so far separated by intervening matters, but the whole may be considered, in a very general view, as one Rock, and as such I shall mention it, in the following description of its range and dip.

The Limestone enters North Wales at Great Ormeshead, resting on coarse Slate, with a NE dip, and so continues through Cefn (on the SW of St. Asaph): and its dip then changing to near ENE, it ranges (speaking of its top) through Denbigh and W of Ruthin, to near Llanelidan; where its edge wheels round, and it ranges NE for about $1\frac{3}{4}$ mile, successively, with a NE, N and NW dip; and making then another wheel, its direction becomes N (with a W dip) ranging E of Garth-gynnan, then near NNW through Llanbedr, Llanganhafal, Llangwyfan, Tra-

* I had more than once heard persons who had travelled this Road, speak of the extreme regularity of *the dip to the north-eastward*, of the coarse Slate, in the Quarry SW side of the Road on Rhualt Hill: but subsequently I have found, that these are only *stratula*, almost at right angles to the plane of the Limestone, when it covered the Slate here: and that wherever I have had the opportunity in North Wales, of examining the junction of these two Rocks, it is far more common to see such stratula in the Slate, than to observe any beds thereof parallel with those of the Limestone.

 merchion

merchion and Dyserth, (having in these last places just a contrary dip to what I had previously understood, and covering the slate instead of passing under it), then wheeling to the NNE and NE, through Melidon, SE of Prestatyn to Gronant, dipping WNW, NW and at length N.

Another grand wheel or change in the direction of these Rocks takes place at Gronant, from whence they range nearly SE, in an undulating line, with a NE dip, somewhat varied locally, in the undulated parts, through Llanasa, SW of Whitford, Holywell and Halkin, to Moel-y-gaer Hill, W of Northope; where a wheel is made to the SW for about a mile (with a SE dip) and then another to the SSE, passing W of Mould, Nercwys and Llandegla to Tafarn-Dowyrch, with an eastern dip, and wheeling then to the SE, it seems to cross the vale and turn N, with first a NE and then a W dip.

From a point SE of Llandegla, about half a mile, some observations are yet wanting, to satisfactorily connect the range of Limestone (or show the course of the *Faults* that prevent such connection) to near Bwlch-y-Gwynt, and the SE side of Minera Chapel, where the Limestone Rock appears again very majestically, with an E dip, and proceeds across Ruabon Mountain and Eglwyseg Rocks, to a great *Fault*, ranging WNW and ESE, which I have traced on the N of Llangollen, passing just S of the great Aqueduct at Pont-y-Cyssyllte and forwards into the Coal-field.

By this Fault, the measures on the S are very greatly thrown up, and the denudated edge of the Limestone, in consequence, now appears a mile more eastwardly: from hence it sets off again SW, with a SE dip, but has not proceeded more than three-quarters of a mile, before another Fault, ranging nearly parallel to the last, again throws the basset to the eastward, into lower ground, where it is nearly covered by alluvia, and this new range continues, on the W of Chirk-Castle, to another great Fault ranging nearly parallel to the former, on the S of Chirk-Castle and the Town; by which the bassets are again thrown eastward: a new range of the Limestone then commences and passes W of Selattyn and near Oswestry Race-ground, to Pentrecefn, where another large E and W Fault, throws up very greatly on its S side, and moves the top of the Limestone E, to within half a mile of Swinney Hall: from which point it again ranges SSW to Llanymynech Quarries, on the south of which, a still larger Fault than any of those above mentioned, ranges nearly NW and SE, and probably passes forwards, on the S of the Wrekin Hill?.

As far as my late hasty view of this part of the Country would enable me to judge, the Llanymynech Fault seems to throw up greatly

greatly on the S, and the Limestone to become surprisingly changed to the southward, in its thickness and qualities, so as to be with great difficulty recognised, when it appears again 4½ m. SE, in Balan Bank, south of the Severn, and whence, as I have been informed, this Limestone Rock, in a thin and very bad state, ranges W of Woolaston and Westbury, N of Minsterley, NE of Haberley and Pulverbach, SW of Leebotwood*, &c. and that a line of small Coal-works (whose names were mentioned to me) accompanies, throughout, this last range of bad Limestone, on its NE side.

The very irregular form and range of the Limestone which has been mentioned above, seems to arise principally, from the numerous inequalities or *ridges and troughs* on the surface of the coarse Slate, on which it was deposited, partly from the subsequent dislocations occasioned by the *Faults*, and partly from the still later and stupendous *denudation*, which the whole country has suffered; and by the combined operations of which three causes, numerous detached lengthened Basins or Troughs, and patches of the Limestone are left, as memorials of their operations.

One of these detached Limestone Troughs nearly crosses Anglesea, from Malldraeth Sands to Red-wharf, and has a small Coal-field in and upon it, see Phil. Mag. vol. xliii. p. 325. Another such Trough ranges along the south-western part of the Menai straight. On the WSW of Caergwrle there are two detached patches of Limestone laid bare, having Coal-measures surrounding them, as I have been informed, and that the most eastern and largest of these denudations (which I have not seen, but wherein Naut-y-Frith Limeworks are situated) nearly resembles that of Ashover, in many particulars.

At Gwerclas on the SW of Corwen, part of a small Basin of the same Limestone is seen, from under the alluvia which conceals the greater part of it, as well as the vestiges of the superincumbent Coal-measures, which the trials on the common NE, are said to have ascertained? On the W of Guilsfield I have ascertained a narrow Trough of the bad Limestone ranging NE and SW, in which some of the lower and barren Coal-measures are found: and through the W end of Welch-Pool Town, Powis-park, and perhaps thence to Montgomery, there ranges the eastern edge of another such Trough of the bad slaty and conglomerate Limestone. Whether parts of similar Troughs of this same Rock

* It has been said, that about 1½ m NE of Church-Stretton, it makes a sudden wheel to the NE, and ranges thus, towards the western side of the Wrekin Hill, with a NW dip?.

appear on the SE side of Bala Pool, NE of Dolgelly, and in several other parts of North Wales, or whether such are strata of Limestone *in* the slate? I have not at present the means of knowing.

I now, consider the identity of the Limestone and superincumbent Coal-measures of Flint, Denbigh and Salop Counties, and those of Derbyshire and Yorkshire, as sufficiently made out, by my recent Survey, striking as the dissimilarities of them appeared at first sight, viz. in the substitution of such very different strata in places of the Toadstones; the extraneous fossils proving much rarer here than in Derbyshire. But the *species* of those that are found, will however agree in those distant places, I believe, both of shells in the Limestone and 9th Coal-shale; and the Crowstone Reeds (W of Oswestry in particular) and other coaly vegetable Impressions. The Limestone-Shale also proves very thin in most parts of North Wales, and either in or under it, an unusual quantity of Chert occurs, locally, in thin strata (sometimes striped as in Ashover and other places in Derbyshire); which siliceous beds and the Limestone near them, seem to be the most productive of Lead Ore, Calamine* and Blende, and perhaps none of the Mineral Veins bear Ore in the lower part of the series, near to the Slate: blue pozolanic Limestone is locally found, imbedded in the lower part of this Limestone Shale. The Coal-shales most of them seem thinner than in Derbyshire; and to the NW of Holywell, useful seams of Coal do not appear below the 5th Grit Rock: but near Oswestry, on the contrary, the seams in the 3rd Coal-shale appear more valuable than perhaps any in this part of the series in or near Derbyshire. In the 10th and 11th Coal-shales (if I mistake not the numbers in the local patches of these, surrounded by Faults) five-yard and three-yard seams of Coals occur, which are thicker than are known in Derbyshire; perhaps the great decrease here, in the thicknesses of the intervening measures, may have brought different seams of Coal in contact?. The Gritstone Rocks in the lower part of these Coal-measures, are vastly thinner than those in the south of Yorkshire, or even in any part of Derbyshire, I believe, and *coarse* Gritstone is much more rare, NW of Holywell; in one spot only ($1\frac{1}{4}$ m. SE of Llanasa) could I discover it, in the 1st Rock, which Rock, in a great part of its range, appears here only as a thin and imperfect gritstone, or as stone Bind beds. The 4th Grit preserves here its usual superiority, as a paving-stone Rock, and in several Quarries on Bryn Llystyn Hill S of

* This is mostly of the *siliceous* kind, which is perhaps unknown in Derbyshire?.

Gwesbyr

Gwesbyr village, paviers are dug, little if any inferior to those of Ealand Edge; the Freestone, Grindstones, Cisterns, &c. from the 3rd Grit in Gwesbyr Quarries, N of the Village near to Talacre Hall, are not less similar to those of many quarries in the same Rock in and near Derbyshire; the Freestone of the 13th Grit Rock in Mostyn Park and other places, proves more superior to the stone of any of the adjacent Rocks, than we find to be the case in Derbyshire: it is here called the Black Cannel Rock.

The Coal-field of Flint (with Wirral in Cheshire), Denbigh and Salop Counties, of which I have been speaking, seems terminated on all its north-east and east sides, by *overlieing*, and sometimes *unconformable strata, of Red Marl,* or their imbedded red, soft Gritstones!; this appears a very striking feature of the British stratification, which I could not see sufficient reason for admitting, *in any case*, until the last Spring, as I have mentioned in a Note to page 330, of your xliiid volume*.

While in Yorkshire, in July last, I was fortunate enough to obtain from my Friend Mr. Andrew Faulds, of Worsborough, an intelligent and able Coal-agent, the sketch plan, section and particulars of Garforth Colliery near Abberford, in which, four seams of Coal occur, in 150 yards of sinking, on the west side of the edge of the yellow Limestone, and in lower ground, dipping S, 1 in 20; and the basset edges of which seams were known for considerable lengths, ranging W and E, directly for the Limestone edge, *under which they pass*, and under the *Sand* stratum also beneath the Limestone!; which sand I have particularly mentioned in Derby. Report ii. 410 Note. Several pits have been sunk in the eastern part of this Colliery, which penetrated the *horizontal yellow Limestone* (with *blue* beds therein I believe) 17 to 20 yards thick, and the Sand beneath it, before *the dipping Coal-measures* and seams of Coals above mentioned were reached!!

This new fact, of which no parallel presented itself, while I was employed on my Derbyshire Survey or since, and of which no author that I have read has mentioned an instance, viz. of the yellow Limestone and its incumbent Sand (or sandstone) being sometimes found *unconformable* to the Coal-measures beneath

* Candour requires me also to mention here, that before examining Pistern Hill, in Smithsby, in the south of Derbyshire, my able friend Edward Mammatt, Esq. of Measham, mentioned to me a report of old Colliers, that *level* Red Marl and Gritstone, there covered *dipping* Coal-measures; but on minutely examining the surface, and *inquiring* on the spot (for the works were long discontinued) I could hear of or see no such thing, and therefore did not mention it in my Report, but which now I regret.

 them,

them*, is very important, and seems to offer, in part at least, a different mode of surmounting the great difficulties, presented by the yellow Limestone border, to the very irregularly wide Coalfield of Nottingham, Derby, York and Durham, than that ventured to be given in 1811, by means of my zig-zag Fault. I must still however be permitted to say, that *proofs* have yet in no instances been offered, that Faults do not range in the manner represented in my Maps, or very nearly so, however much I may have miscalculated the magnitudes of their derangements, and consequent effects on the visible parts of the Strata: and that even in the present instance, that stretch or line of the zig-zag Fault, which I had observed passing (from Thorner) in a NW and SE direction, at the foot of the Limestone edge near Kidhall, on the Road from Tadcaster to Leeds, is recognized and named as such, in the Garforth Colliery map, received from Mr. Faulds; although a far less derangement seems there assigned to it, than I had supposed.

In hastily examining last November, the Coal-measures at Crew-Green or Bausley Colliery, (2 m. WNW of Alberbury, which is in Salop,) I observed them declining E, under Red Marl Grit; to which succeeds a thick Limestone Rock stretching from Alberbury to Caerdiston, with a NE dip, the Limestone of which, partakes of that extraordinary conglomerate character, that I have already mentioned, as distinguishing this district. It must however, I think, be referred to *the yellow Limestone Rock*, imbedded in the vast Red-marl Grit, which extends thence to Shrewsbury, and further; and it would be desirable to know from an analysis, by some of your chemical Readers, in what proportion Magnesia may be a component of the Alberbury Limestone Rock ?.

The *Corn-stone*, which is frequently a conglomerate sort of Limestone, on the eastern side of the Clee Hills, at Barnaby, Ince, &c. is imbedded in Red Marl or its Grit, and such appears now to me (on reconsidering my observations made in July 1812) to belong to the unconformable yellow Limestone Rock, and its imbedding Red Marl.

The Limestone Trough in the vale of Clwyd, which I have

* In justice to my able and respected friend Thomas Walker, Esq. of Eastwood, and others of the Coal-Masters, alluded to at bottom of page 166, of my Derby. Report, vol. i. and P. M. vol. xlii. p. 110, I ought to repeat here, the opinion entertained by them, that the Bilborough Coals emerged from under and left the edge of the yellow Lime, appearing, *when out beyond its edge to lie unconformable therewith.* But none of these Gentlemen or others mentioned, or even I believe hinted at, an unconformableness, *proved* between the Coals and superincumbent Limestone, in any of their numerous pits in Nottinghamshire, sunk through the latter rock.

described

described at page 163, undoubtedly has the lower part of the Coal-measures in it, since they appear at Nant-y-Felin Farm, 1½ m. WSW of Llanganhafal, resting on the Limestone, and in other places I believe; but these Coal-measures are in most parts concealed, by *an unconformable covering* of Red-Marl Grit, or by an *alluvial covering* of Clay and Bolders upon this, in many places; and which unconformable and alluvial coverings, in many places concealing the edge of the Limestone also, and causing it to appear in isolated Rocks, have presented the obstacles to the true understanding of the structure of this curious valley, to which I have already alluded in page 163.

If time would permit, I might now adduce alike corroborating instances, from numerous other parts of England, of the unconformableness of that almost universal covering of Red Marl, to all the middle and western parts of South Britain, which I mentioned in a paper read to the Royal Society in the spring of 1811*. At which time, the unconformable character of the yellow Limestone and sand or grit, below it, was not known, or its relation to the Red Marl understood; and I was likewise under the mistake (derived from Mr. Smith) of supposing the situation of the Somersetshire or upper Coal-measures, (vol. i. pp. 132, 159, 174 and 179 of my Derby. Report,) to be *above* the yellow Limestone, instead of *below* it; and instead of considering this last Rock as only occasionally found, among the anomalously imbedded substances of the Red Marl, which I had endeavoured to enumerate in my Derby. Report, i. p. 148.

The *Geological Inferences* then, which appear to me deducible from the above, are, that the British series of Strata is reducible into two very distinct parts, the product perhaps of very distant periods of time; the lowest or first, in which *coarse Slate*† and numerous other substances imbedded in the same, were deposited, and left with a very uneven surface; then *Limestone* of very variable thickness and separated locally into distinct Rocks, by intervening matters of very different natures; then a vast series of *Coal-measures*, very improperly, I think, called "Independent,"

* See the xxxixth volume of the Phil. Mag. page 28.

† Many, I am aware, will be ready to exclaim that this is beginning at the surface only of the crust of the Earth, while *their theorics* enable them to descend to older and still lower Rocks, in exact order, to the very centre of our Globe:—be it so, provided they can account for its total specific gravity? I will however take the liberty of saying, that their *geognostic dogmas* afford no helps towards usefully exploring and understanding the British Strata which are accessible, but on the contrary, have most materially impeded the knowledge to be obtained, on *Smithian* principles, of the actual structure of our Country, as deep as it concerns us materially to know it, and from whence important benefits would flow to Science, and to the arts and property of the kingdom.

pendent," by numerous modern writers, because the evidences seem very conclusive, that this formation was uniformly spread over the Limestone, as that also was over the Slate, before the first stupendous *denudations* took place, by which large patches of these strata were entirely removed, and their fractured edges exposed on all sides.

Some of my Friends are lately inclined to think, that this state of external violence on the older strata, terminated by the deposition, locally, of vast masses of highly mechanically *rounded* Quartz and other pebbles; but I must see clear evidence, of such Gravel being now found *under* any strata, those of Red Marl at the least, before I can assent to this position, or cease to assign a much later period, to *all the Gravel and alluvial matters* that we are acquainted with.

The second grand division or period of the British stratification, seems to have commenced with the deposition of the Red Marl, and its various locally imbedded Substances*, *in an unconformable manner* on all the denudated *edges* of the older strata, but *conformably* on all their naked *planes*; and as these last greatly preponderated in extent or surface, it has happened, that this general unconformableness, has not yet been universally known and assented to by British observers.

Conformably with the top of this vast series of Red Marl†, and its imbedded soft red Gritstones, &c. are found deposited, the vast series of strata of the east and south-east of England and the adjacent parts of the Continent‡, as I first, I believe, enumerated them, in print, in my Derby. Report, page 111 to page 116, but in the reverse order to that in which we are now considering them: and on which enumeration of the upper British Series, it seems now proper to remark, that the "natural division" of the whole of those mentioned, will seem made with more propriety, at the *bottom*, rather than the top of the Red Marl Series. I would also here recall to mind, the correction which I made at

* The Basaltic hummocks, so often found locally covering the Coal-measures, seem referable to the bottom of this stratum, and are perhaps unconformable in some instances?.

† The irregular and wedge-like stratification, so common in the Gritstones of this stratum, sufficiently explains how the stratification, begun on an uneven floor, was reduced above, to the very regular and *horizontal* beds, which I have mentioned in my Report, vol. i. pp. 147 and 174, and which I have since almost everywhere observed in this Marl.

‡ We have been made acquainted with the identity of the upper part of the series of Strata, in these different Countries, by investigations commenced abroad, *since* Mr. Smith's investigation and Maps had been matured, nearly to their present state (as I have explained in your xxxvth volume, page 113), and had been made known to scores of intelligent persons, although to the present time, *he* has published nothing on the subject.

page

page 100 of your xxxixth volume, of my previous statements, as to real Coal-seams not being found in England, higher in the series than the Red Marl, as Mr. Smith originally taught.

Since *the deposition* of the upper series of British strata was completed, the same seems to have been subjected to *dislocating*, and since to *denudating* external causes, more extensive in their operation than those before mentioned; and wherein the effect of *wise design* is throughout clearly manifested, in so directing the stupendous excavating cause (or causes) as to produce the perfect *system of valleys*, covering *every part of the surface*, for its drainage, as completely as the veins are distributed for returning all the blood to the heart of an animal. Towards the conclusion of which eventful period, depositions of *alluvial matters* were generally scattered, and in some places accumulated in vast heaps, filling up slight inequalities, and finishing the beautiful contour of the terrestrial surface, as the same had previously been *designed*, by its Creator. The *still operating* causes, of decomposition or mouldering of the surface of the strata, the tides, torrents, depositions from turbid waters, the growth of peat, &c. &c. have in so very trifling a degree altered the true geological features of the country, as they were left at the period above mentioned (although the same appears to have been long prior to the existence of *all the present races of living animals and vegetables*) that the mention of them here, might perhaps have been spared.

It remains only, to hint at the very important consequences, to the present and to future generations of the Inhabitants of this Island, which may follow from the views here taken, as to *the probable extent of Coals*, under the greater part of the Red Marl!; and that a minute examination of the extended surface of this stratum, with a view to discover such parts as are *deepest excavated*, or wherein the under-measures are brought nearest to the surface, by irregularities in their planes, or by Faults, wherein trials for Coals may be instituted, with prospects of the greatest advantage to districts, that are now far removed from pits of this essential article, are among our greatest desiderata.

I am, sir,

Your obedient servant,

Westminster, Feb. 18, 1815. JOHN FAREY, *Sen.*

A Postscript to the above Letter.

SIR,—I CONFESS myself well pleased, that my Letter on Geological subjects, transmitted to you on the 18th ult. could not be admitted into your last number, since having read the first article

article printed therein, because of the opportunity that you will I hope now afford me, of adding some further observations, by way of Postscript to my Letter, which I will now proceed to make.

I have never visited the neighbourhood of Whitehaven; but in several conversations which I had in the spring of 1806, with Mr. Edward Martin, who furnished materials for the account published that year in the Philosophical Transactions, of the Coal-Basin of South Wales, wherein he has long resided and practised as a Coal-Engineer, I remember his telling me, that he was brought up and taught his art, in the Coal district near Whitehaven, and that the several Coal-seams there, could be clearly identified with those in South Wales, where he was then more immediately employed. In his account to me of the latter Coal-Field, in the published account thereof, or in any of my subsequent inquiries while on the edge of this district or otherwise, I have not been able to learn, that Limestone occurs in the South Wales Coal Basin, except at the lower edges of the Coal-measures? basseting from under them: I wish now therefore to supply an omission in my Letter, where I should, when enumerating the probable *instances of unconformable yellow Limestone on Coal-measures*, (p. 168) have mentioned that at Preston-Hows, 1½ m. SW of Whitehaven, twelve yards of yellowish *Limestone*, and then 43 yards of red Grit Rock, are sunk through at the top of a Coal-Pit, that is 216 yards deep!; the minute particulars of which were long ago published by Mr. Dixon, and have been copied into the 2d Edition of Williams's "Mineral Kingdom," vol. ii. p. 246; and I am anxious to lose no time, in proposing the addition of this important fact, to Mr. Bakewell's Section* at top of your 2d

* This *Section* by Mr. B. must from his description be supposed to pass from ENE to WSW, from near Sunderland, passing not far from Penrith and Keswick, to near Whitehaven, as otherwise, it could not cross the coarse Slate mountains near the Lakes (p. 90); a person travelling this line, could not fail to see and cross the remarkable *Limestone Rock*, ranging NW with an eastern dip, through Shap, Lowther, W of Penrith, through Graystock, &c. between the no less remarkable *red Sandstone* of Penrith and the *Slate* mountains; round all the eastern, northern and western sides of which slates I believe this Limestone to lap, but which we search for in vain in Mr. B's Section!; and wherein it would I believe be found, that between C and the Slate, considerably too little space is allowed, and that *a trough* of Limestone is there omitted, containing *Coal-measures* within it. (having small Coal works locally scattered thereon, of which I gave Mr. B. a hint in your xliid vol. page 57) under *an unconformable cover* of Red Marl, containing Gypsum (near Culgaith, &c. Forster, p. 44,) and Red Sandstone, &c. locally distributed thereon: and which over-licing "*old* red sandstone," of the *Geognosts*, has, as I believe, been mistakenly represented by Mr. Forster, as passing under the strata of his Section; whereas it will more likely be found, that the lower and calcareous parts of these strata are made to approach each other, in or westward of the Cross-Fell range of Mountains, by the thinning and almost total disappearance of the intervening strata, and growing thinner themselves also (as happens on the NE side of the vale

2d plate; and also of soliciting the assistance of your Correspondents, for ascertaining two very interesting facts, viz. 1st, Is the Preston-Hows Limestone *Magnesian?*; and, 2dly, Is it, and the grit below it, *unconformable* in any part, to the Coal-measures and seams of Coal below them?.

I would also beg to solicit here, the careful consideration by my able Friend, Mr. David Mushett, of what has been stated above, and to request a further communication from him to you, as to the yellow Limestone covering Coal-measures near Newent, in Gloucestershire, mentioned in your xlth volume, p. 54:—Whether this Limestone (ranging I believe almost uninterruptedly

vale of Clwyd, p. 163, and numerous other places) when they begin to bend down into the Trough between these last mountains and those in which the Lakes are situated.

I beg further to mention concerning this Section, that while in Durham County, I did not see, or hear from Mr. Arrowsmith and other practical Colliers with whom I conversed, of any Coal-seams which regularly *basset towards the East*, instead of continuing on and passing under the yellow Limestone: I hope therefore that Mr. B. will favour me and your Readers by a little more precision, as to the actual line of his Section in this part, and the truth of its representation, to the left or westward of his letter B. I heard of the Coal being in some places cut off, and *cropping out* before it came to the Limestone (p. 84). But on inquiry, the former of these seemed only ordinary cases of faults, and the latter, as far as they were not altogether supposititious, seemed only local *risings* of the strata against bumps in those beneath: but no final *bassets down hill* were mentioned, such as the Section represents.

At the bottom of page 90, Mr. B. appears to apologize to your Readers, for making a quotation, "in the language of hypothesis," from Mr. Forster's work, (p. 126); it had however I think been better for Mr. B's reputation and the cause of truth, that the whole should have been omitted; since in searching through Mr. F's work, not a sentence will be found of his to countenance the *Plutonic* idea, of Mountains being "thrust through the superficial covering of the Globe," by imaginary subterranean Fires!. The following is an essential part of the sentence which Mr. B. has *partially* quoted, following the words "Scotland and northward;" viz. "The mines at Leadhill and Wanlock-head are in this *rock*, as are also the (Black-lead) Wad-Mines in Borrowdale, and several other mines of Lead, &c. in different places. Blue Slate, the elegant covering for Houses, is quarried out of this *rock* in several places of Cumberland." If we turn to page 22, Note, we shall find Mr. F's own definition of the word *dyke*, used at the beginning of the passage quoted, (not "dyke or vein" as Mr. B. previously intimates,) to be, "a *natural crack*, fissure, or chasm in the strata, which chasm is commonly filled up with *heterogeneous* matter." All which I think fully exclude the interpretation that Mr. B. has attempted, of this passage in Mr. Forster's Book; the obscurity at the beginning of which passage, I have been used to attribute, to an error of the press, in wrongly joining an unacknowledged quotation from some Author (of which unfortunately Mr. F's Book furnishes numerous instances) in the middle of his own account of dykes of stone.

The unfortunate figure which Mr. B. must have cut, from indulging in these his favourite Plutonic whims, as to the thrusting up of the mass of the Cleveland or eastern Moorland Hills, and his consequent *inference* of inclining strata from their centre, contrary to the facts, (see Phil. Mag. vol. xliii. pp. 257 and 258,) would, I should have thought, operated, to prevent similar indulgences again, so near to the same district. I had entertained the hope, that the forthcoming Edition of Mr. B's Geology, would be freed from these Plutonic blemishes, and wish I may not be disappointed on its appearance.

from

from Flaxley-abbey to Abberley) is not unconformable?, in some places at least, &c. &c.

I wish next to mention, that having returned to Mr. Faulds the rough Sketches of the Garforth Colliery, Map and Section, which he had lent me (p. 167), with reduced copies thereof for his careful revision on the spot, and requesting answers to 17 quéries on the subject; that a few days ago I was favoured by his answer, mentioning, that his valuable Friend Mr. James Porter, of Garforth, had first pointed out to him the very interesting, although not *unique* circumstances of this Colliery, because he says, that nearly similar observations have been made in Kippax and in Glass-Houghton Collieries, a few miles southward:—that he had transmitted my Map and Queries to Mr. Porter, from whom I might expect shortly to receive more satisfactory answers, than it was at present in his power to give, respecting a district near 20 miles from his own residence. I may therefore hope to have the means shortly, of bringing these important Facts more fully before the public.

I have never visited either Kippax or Glass-Houghton Collieries, although I have made some observations on the gypseous Red Marl between the Limestone Rocks at Fairburn, which is between these places, as mentioned in your xxxixth volume, p. 104; and I cannot avoid on this occasion expressing my surprise and regret, that neither during this excursion, nor in the large mass of written information which my kind Friend Mr. Smithson collected for me, in answer to queries which he printed and circulated to most of the Yorkshire Coal-Owners, &c. (and which command and have my most grateful acknowledgements to those Gentlemen) that these interesting facts were not sooner made known. I can now pretty clearly see the reason, and it may be of use to other observers to mention it, of my never having noticed or suspected the unconformableness of the yellow Limestone to the Coal-measures, viz. the loose nature of the intervening *Sand* or soft Rock, which will in general be found mouldered down over the upper Coal-measures: otherwise, in the numerous times that I have ascended and descended the edge of this Limestone Rock, in various parts of its range, I must have noticed this fact (if as prevalent as I now suspect it to be), if the Limestone had lain immediately on the Coal-measures. I hope however, now, that the attention of those best able to elucidate the subject, I mean the *practical* Men of the district, and of every other, where this Rock can be identified, will not relax, until the truth, wherever it may lie, is fully established.

It will be seen by your Readers, that Mr. Bakewell and myself now concur pretty fully, as to the little or rather the *negative* aid that

that we may expect, either from the initiated or the dupes of a foreign *Geognostic* faction sprung up amongst us; and your work, Mr. Editor, is the more valuable, as a channel of communication *for useful facts*, whether new or in confirmation or correction of others already printed, expressed *in the terms and language* which may be familiar to the writers or their informants, accompanied or not by *Inferences*, of any kind, which they may think deducible from their own communicated information, or that of others.

I could not help thinking this morning that I perceived something of the influence above alluded to, while perusing an account just published in the "Annals of Philosophy," of four species (or varieties rather perhaps, some of them) of the remains or alveoli of *Belemnites*, found in argillaceous Coal-measures, and of six others found in the Limestone near adjacent to them*,—in Blackburn Colliery in Livingstone, in Linlithgowshire:—how else could it be doubted (p. 204) that "the concamerated alveolis" belonged to a Belemnite!, or the mistakes have now been printed, that Belemnites are *very rare* in Scotland?, and much more so, that in England we find them in few situations?: when Mr. James Sowerby in his "Mineral Conchology," p. 128, has mentioned the proofs that exist, in Woodward's and Bevan's Specimens and others, and alluded to his own and others' specimens, which show this incontestably. While I was in Scotland, I saw at Brora, very numerous Belemnites, in what I believe to be the very same Coal-shale, as that from whence Mr. Fleming took his specimens: they were well known there, by the name of "petrified Tangles" (a tubular sea-weed) as mentioned by another Scotch observer in the same number of the Annals, p. 235, and were considered as far from uncommon in Scotland. In England we have at least 20 strata (far separated from each other, by intervening strata without Belemnites) which produce them, probably *of a distinct species* in each Belemnitic stratum, and even that more than one species are found, in some of these places in the British series; so that excepting anomia Shells, we have perhaps no extraneous Fossil near so common in England as the Belemnite, and in nearly all of which, when *in situ*, vestiges at least of their alveoli may be seen.

The Gentleman alluded to, furnished Mr. Sowerby with five species of shells of the genus *Productus*, from either the same or a near adjoining Limestone Rock as his six Belemnites (or "Orthocera," as it seems now agreed to call them) were taken, and which are published in No. 13, of Min. Conch. I presume to hope therefore, that either he, or some kind friends to Geological

* See Kirwan's "Geological Essays," p. 314.

Investigation,

Investigation, will furnish Mr. S. with well selected *specimens* of all these Orthocera, from as many and as distant places as may be, mentioning their exact localities and strata, by their ordinary and local names, in order that the same may in due course appear, in his very useful work, unmixed with Geognostic speculations and distinctions, which may greatly impair, but cannot add to the value of coloured figures and a descriptive enumeration of British Fossil Shells, with their habitats.

But to return to the first article in your last Number, I beg to congratulate Mr. Bakewell on the very superior account of the Northumberland and Durham strata, which he has presented to your Readers; and to thank him for the very liberal manner in which he has brought Mr. Forster's work before the public *, and which I hope that we shall see reiterated in the forth-coming Edition of his Geology. And consistently with the opinion that I have long ago expressed to your Readers, of *the usefulness* of Mr. B's Geological writings (and which, from being *less Geognostical and Plutonical*, will I trust further improve hereafter), I beg to make a few further remarks on his late paper, with a view only of adding to its truth and usefulness.

It will be recollected, that I objected to Mr. Bakewell's Geological Map of England, in his Geology, 1st Edition, as attempting *a greater simplification than nature admitted*; at that time Mr. B. did not extend his "Low district" far enough north in Yorkshire (as I observed, P. M. xlii. p. 57), but now on the contrary, he seems to have extended this uppermost of his three divisions, too far in that direction, when he intimates, at p. 82, that the yellow Limestone (extending far into Northumberland) is to be classed with the strata that prevail "on the eastern side of England †;" notwithstanding that this Rock so evidently *belongs to the Red Marl*, which is nowhere admitted into Mr. B's low district, N of Leicester; but even the Bath Oalite, far above it, is excluded a place therein, in Mr. B's Map, in the vicinity of Bath, and thence southward.

The grand *Fault* which I have supposed to range SW and NE from near Ainderby-Steeple to near Redcar in Yorkshire, (P. M. xxxix.

* I have very often quoted, and recommended the sale of Mr. Forster's Book; but unfortunately, it is not sold but at *Newcastle*, whence I have procured it, for three or four of my friends. I beg also to mention, with the view of serving Mr. F. that if he would cause it to be advertised and sold in London, it would soon be more sought after; and from a similar motive I will mention also, that the Binder employed to do up his Work in Boards, most reprehensibly omits *sewing* therein, so that all the copies I have seen, were no sooner cut, but the middle of each sheet became loose and ready to fall out.

† In page 93, Mr. B. expressly says, that the Sunderland Limestone extends "through the *south-eastern Counties*," but which surely is unfounded.

xxxix. p. 95), seems now more than ever necessary, for accounting for the Bath Oalite, the Lias Clay and Limestone, and numerous other thick strata, nowhere appearing on our east coast!: and since that Mr. B. has at length admitted (p. 93) "the frequent disappearance of a whole series of strata on one side of a Fault," I hope that in his new Edition, he will not be found supporting the very improbable position he has mentioned p. 84, viz. that the alum Shale (or clunch Clay) of the Cleveland Hills, is identical with the Coal shale, intervening between the yellow Limestone and the Coals at Pallion, 2 m. W of Sunderland Iron Bridge: the thickness of which Coal shale, I cannot think but Mr. B. has over-rated, by trusting to his memory; and on which account I beg to express a wish, that Mr. Goodchild would favour you with an accurate and particular account of his boring for Coals under the yellow Limestone, to be recorded in your work. Considering, that at Ferry Hill Coals have long been worked, on both sides of the Turnpike Road, and within sight of it, at Pits *sunk through the yellow Limestone Rock**, I have been surprised to see Dr. Thomson (Ann. iv. 415) expressing his ignorance of any such pit sunk through this Limestone; and even more so, at the doubts that Mr. B. seems to express of the same fact, at page 84 of your last number.

March 1, 1815. J. FAREY.

XXX. *On the Phænomena attending the Roots of Plants in snowy Weather.* By *Mrs.* AGNES IBBETSON.

To Mr. Tilloch.

SIR, — HAVING seen a letter in one of your Magazines, endeavouring to account for the phænomenon attending the roots of plants in snowy weather, and being humbly of opinion that the fact is mis-stated, having last winter particularly attended to the appearance in question, I shall venture to suggest a few observations on the subject.

Whenever the snow lies for any time, generally it is observed to melt and pass off round where a tree or shrub *shades* the *ground*. Now in reality this effect has nothing to do with the tree, as I shall show. At first, indeed, it occurred to me that the motion of the root might occasion a degree of heat which might melt the snow, as I know by the dissection of roots that there is much more action in them than is generally supposed,

* A circumstance which I have mentioned in your xliid vol. p. 53; and which Mr. B. has noticed at the top of page 124, but only for commenting on an immaterial circumstance mentioned by me.

and motion will prevent congelation even in water, if not already too near the freezing point. But observing that the snow passed off in breadth in proportion to the spreading and shading of the branches; I fixed a long stick in the ground, and tied an umbrella to it, which should, when spreading, be about six or seven feet from the earth. It was then going to snow; and when it was already pretty deep round the stick, I expanded the umbrella, and before the next morning, the whole extent of the circle of the parasole was totally melted before nine o'clock. It could not therefore be any effect of the roots of plants that caused the snow to dissolve; but was it not rather the dews and vapours falling and sinking among the dead leaves, putrid vegetable matter, or calcareous earth, which became nitrate of potash or saltpetre, and of course increased the cold, and preserved the congelation of the rest of the snow? Many other trials and experiments have since that time confirmed my *conviction of its propriety*; though not of consequence enough to be *submitted* to the *reader*. I cannot help thinking that we are *little aware how many juices*, and various *liquids* and compounds, are bestowed on us by the atmosphere. The hairs of the plants make that subject *most visible*, though they may, like the salts aforementioned, not descend to us in a *perfect state*; yet they certainly (with the liquid already in the hairs, or rising immediately after *in them*) receive *affinities* which produce most of the scents and juices that form our fragrant oils and resins.

I am, sir,

Your obliged servant,

Sherwood, Feb. 3, 1815. AGNES IBBETSON.

XXXI. *On the Rate of going of two Clocks, with Remarks on* HARRIS'S *Pendulum Clock erected in* 1641. *By Mr.* THOMAS REID.

To Mr. Tilloch.

SIR, — THE circumstance of two clocks keeping the same time so closely together, and that for periods of considerable lengths of time, appearing extraordinary and unexpected to me, is the reason why I have transmitted the case to you; and should you deem it worthy an insertion in your valuable Journal, it is at your service.

Both the clocks were going in an imperfect state, and were intended for further improvement: but their assuming the appearance of keeping so near together, made me delay the taking them down to make any alterations, till I saw how long they would continue to do so; which they did for such a length of

of time, and might have done so *for how long* I shall not pretend to say; had it not at last became necessary for me to put an end to it. One of the clocks was a month one, with a recoiling scapement, and a compensation pendulum made after Ward's construction. The other was an eight-day one, having a detached scapement, and a zinc tube compensation pendulum, both made some years ago, and of my own contrivance.

During a period of four months previous to the middle of November 1813, they kept so constantly and close together, that at no time could I perceive the tenth part of a second of difference between them; indeed no sensible difference could be perceived, either by myself or by another person who had occasion frequently to see them expressly for this purpose. A pretty sharp cold taking place on one of the nights in this November, the month clock made a little deviation from the other: this was imputed to the influence of the cold upon the oil, as the arc of the vibration of the pendulum was followed by being shortened a little at this same time. However, very soon after, they again went together, allowance being made for the difference or deviation made by the one from the other, and this difference was kept up to for *more* than a period of 164 days afterwards. The mean daily rate of these clocks, during the four months previous to the middle of November 1813, was +0·2 per diem. The mean daily rate of them during a period of 164 days, viz. from the 2d of February 1814 to the 16th of July, was also +0·2 per diem, the same as before. I shall give an abstract more in detail, of their going during these 164 days, not with the view of showing any great perfection either in their rates or in the compensation; yet at the same time it may be allowed that in these respects, considering the incorrect state in which they were, few clocks, even in their best and most correct condition, have, *if any thing,* much exceeded this. On the 2d of February, the clock having the detached scapement was fast by mean time $1^m\ 35^s$. The month clock was about 53 seconds faster than it, and this difference it maintained during 164 days, when the month clock was taken down to be corrected and to get another pendulum.

			No. of Days.	*Total Gain.*	*Mean daily Rate.*	*Mean of the Thermo.*
February	2.	$+1^m\ 35^s$				
March	7.	+1 46	33	11	+0·33	40
—	29.	+1 50	22	4	+0·18	42
May	11.	+1 55	48	5	+0·11	55
June	10.	+2 4	30	9	+0· 3	52
July	16.	+2 10	36	6	+0·16	60

The intermediate times of trial gave them for the greatest

M 2

mean

mean daily rate $+0^{s}\cdot33$, and for the least $+0^{s}\cdot11$, and the mean of these five periods is $+0^{s}\cdot216$ as the mean daily rate.

These two clocks coming to keep time so close and near together, and for such a length of time, was a matter merely accidental: I should consider it a very vain pursuit, to attempt to make any two clocks do so, nor do I think that there is any probability of my seeing the like to take place again. It is not impossible to make two clocks to go very nearly together, and for a considerable length of time *too*, but in this *very nearly*, and in that *quite close*, there is a very wide difference.

It has been said in some of the numbers of your Phil. Journal, where I think that I have seen it, that a clock with a dead beat scapement was made by the late Mr. Grignion, and given by him to the Society for the Encouragement of Arts, Manufactures, &c. "that any addition or diminution of the motive force would produce no alteration in the time-keeping of the clock." Now, sir, being well acquainted with the principle of the dead beat scapement, I cannot easily admit this. When this circumstance was mentioned, it should surely have been stated in what manner this was obtained by Mr. Grignion, in the dead beat scapement. Where this takes place in any scapement, I humbly presume it can then no longer have the properties of that of the dead beat scapement. However, if you or any of your correspondents will have the goodness to take the trouble to explain this, it will be esteemed a considerable favour.

From the same respectable authority of Mr. Grignion, we are informed that in the year 1641 a clock was made and put up in the church of St. Paul's, Covent-garden, by a Richard Harris, who had applied a pendulum to it. This is rather singular, considering the cavilling which took place many years afterwards, as to the priority or right of having first applied the pendulum to a clock. Father Alexander tells us, that there were no clocks made with pendulums to them in Paris, till after the year 1660; and yet there was a duodecimo pamphlet of Galilei's, On the nature and properties of pendulous bodies, translated from the Italian into French, published at Paris in 1639. May we not hazard a conjecture for Richard Harris in this case? Our countryman Inigo Jones travelled twice into Italy, and was at Venice, about the time that Galilei was there. It is not improbable that Mr. Jones when there may have heard of what Galilei had suggested regarding the pendulum, and of the propriety of applying it to a clock, and afterwards may have communicated these ideas to Richard Harris on his returning to London. It must be observed, that Inigo Jones was not in life at the time when

when the dispute on this subject took place between Hooke, Huyghens, and others. Whether Richard Harris was, I know not.

I am, sir,

Your most obedient humble servant,

THOMAS REID.

XXXII. *On Steam-Boats. By* ROBERTSON BUCHANAN, *Esq. of Glasgow.*

To Mr. Tilloch.

DEAR SIR,—AGREEABLY to your request, I now send you some account of the steam-boats on the Clyde.

I am, dear sir,

Your most obedient servant,

Glasgow, January 9, 1815. ROBERTSON BUCHANAN.

So early as the year 1801, a vessel propelled by steam was tried on the Forth and Clyde inland navigation, but was laid aside, among other reasons, on account of the injury it threatened the banks of the canal by the agitation of the water: and as far as I can learn, the same objection still subsists to the use of steam-boats on artificial canals so narrow as those usual in Great Britain. That objection, however, I should think, does not apply to some of those of Holland and other countries on the continent.

The first attempt on any scale worthy of notice, to navigate by steam on the river Clyde, was in the year 1812*. A passage boat of about 40 feet keel and $10\frac{1}{2}$ feet beam, having a steam-engine of only three horses' power, began to ply on the river. Since that period the number of boats has gradually increased.

Besides three vessels which have left the Clyde, there are six at present plying on the river, two of which carry goods as well as passengers. They have on the whole been gradually increased in tonnage as well as in the power of their engines; and still larger boats and more powerful engines are now constructing: among others, one of about 100 feet keel and 17 feet beam with an engine of 24 horses' power; and one of equal burthen, having an engine of 30 horses' power †. These boats are all neatly fitted up, and some of them even elegantly decorated.

On board all the passage steam-boats are newspapers, pam-

* The first steam-boat in America was launched at New-York on the 3d of October 1807, and began to ply on the river between that city and Albany, a distance of about 120 miles.

† For the value of a horse's power, see Buchanan's Essay on Mill-work, Teeth of Wheels, p. 130.

 phlets,

phlets, books, &c. for the amusement of the passengers, and such refreshments as are desirable on so short a voyage, a distance of about 26 miles by water, and 24 by land.

The voyage betwixt Glasgow and Greenock, including stoppages at intermediate places, is commonly accomplished in from three to four hours, the vessels taking advantage of the tide as far as circumstances will permit: but as they start at different hours from the same place, they are sometimes obliged to go part or nearly the whole of their voyage against the tide.

The voyage has been accomplished in $2\frac{1}{4}$ hours; the tide being favourable, but against a moderate breeze of contrary wind *.

At first, owing to the novelty and apparent danger of the conveyance, the number of passengers was so very small that the only steam-boat then on the river could hardly clear her expenses: but the degree of success which attended that attempt soon commanded public confidence. The number of passengers which now go in those boats may seem incredible to those who have not witnessed it. Travelling by land has not only been nearly superseded, but the communication very greatly increased, owing to the cheapness and facility of the conveyance. Many days, in fine weather, from 500 to 600 have gone from Glasgow to Port-Glasgow and Greenock, and returned in the same day. One of the boats alone has been known to carry 247 at one time. The increase of travelling in consequence of navigation by steam, may be estimated by the number that went in the common passage boats before the introduction of this agent: at that time, the highest estimate even for summer did not much exceed 50 up and 50 down, and those generally of the lower class of the people. The number that then went by coaches may be thus estimated: four coaches up and four down, which might average six passengers each.

In the summer, the pleasure of the voyage and the beauty of the scenery attract multitudes; and the bathing-places below Greenock have, in consequence of the easy passage, been crowded beyond former example.

The scenery near Glasgow is sylvan and beautiful, but becomes bolder and more picturesque as the river descends, until it terminate in the rugged mountains of the west Highlands.

ROBERTSON BUCHANAN.

General Description.

A variety of modes of propelling steam-boats by the power of steam-engines have been projected, and many of them tried: but

* The time which was allowed to the Mail coach to go between those towns was $3\frac{1}{2}$ hours, but owing to extraordinary exertion some of the coaches now run that distance in about $2\frac{1}{2}$ hours.

those

those on the Clyde have their machinery all constructed on one general plan; namely, that of paddle-wheels similar to undershot water-mill wheels on each side of the vessel, which are put in motion by the steam-engine.

Reference to Drawings of Steam-Boat.

Plate III. An elevation; a side view showing one of the paddle wheels.

IV. A plan, showing the extent of cabin floor.

A. The fore or second cabin.
BB. Space for the machinery.
C. The iron chimney, serving also as a mast.
D. The boiler.
EE. The steam-engine.
G. The crank.
H. The fly-wheel.
II. The paddle-wheels.
K. Ladies' cabin.
L. Steward's room.
M. Principal cabin.
NN. Stairs down to the cabins.
OO. Water closet.
PP &c. Gangway.
QQQ. Seats at stern and on the deck.
R. The rudder.
S. Covering of paddle-wheels.

XXXIII. *A Paper proving that the Embryos of the Seeds are formed in the Root alone.* By Mrs. AGNES IBBETSON.

To Mr. Tilloch.

Sherwood, Feb. 24, 1815.

SIR,—THAT time is now arrived, of which I last year promised to give notice, when the seeds are to be discovered mounting in the alburnum vessels from the root. It is a phænomenon so *easily seen*, that I cannot help calling on botanists in general to convince themselves of a fact of such consequence to science, and establishing the foundation (if admitted) of a more perfect knowledge in the formation of plants than we yet possess. It requires no other preparation (to view it well) than merely cutting off a small piece of the outward rind of any tree, then cutting an extremely thin slice adjoining the several cuticles, which if it is the proper piece (that is the alburnum vessels) it will be so soft as to cut with the utmost ease. In this specimen, with the naked eye, if held up to the light, but certainly with a small magnifier, the seeds will be seen mounting the tree. I shall first give a

 complete

complete account of their proceedings from their first formation in the side roots to their settling in the buds: I shall then answer every objection that has occurred to myself, or been suggested by others in contradiction to the fact here reported, with the same exactness and impartiality as if I were unconnected with the discovery.

When first I viewed these balls just entering the bud, I could not conceive what they were; but pursuing them in the *right season* for *several years together*, I found that they commenced their course in the radicle, at the termination of the side roots, about the end of January; *there* they appeared to be first formed in a sort of *gross powder*, which separated as it advanced further into the root, and soon became very small balls, which afterwards entered the narrow passage of the middle root: here they generally stopped for a time, and then, proceeding across the centre, entered the alburnum vessels in the stem, and mounted to the buds. Suppose the larch or oak tree,—but the first is the most distinguished and clear for viewing the completion of this curious phænomenon, as the shooting of its beautiful red flowers marks best the time of observation. The seeds having mounted the stem arrive at a collection of gemmæ, and form a large heap at the middle points of the pith leading up to the buds; here they remain many days, perhaps a week or more, till the vessel of dispersion has formed, and run from the heap opening at each bud; the seed-vessel of which remains *distended* for the reception of the seeds. When this is complete, the balls enter this *new-formed vessel* one by one, and slide up the cylinder to each pericarp, and such a number of balls are deposited in each seed-vessel as suits the order to which the tree belongs. Thus the seeds disappear from the heap by degrees, and the pericarps when they have attained their proper number close at the bottom, and the vessel of dispersion is soon lost in the increasing part of the plant; but the seeds never enlarge from the time they quit the middle root till they enter the bud.

I must observe that I have every reason to believe that it is the *heart* of the seed only that is formed in the root, that part which *afterwards becomes the embryo* of the plant. In the wheat and grasses it is so exactly *marked*, as the heart is before impregnation, that it is impossible not to be struck with the similitude of the figure. I conclude, therefore, that this part is formed by the immediate assemblage of the fresh blood of the plant mixing with and imbibing the new sap just proceeding from the earth, and pouring into the side roots. We know that the liquid of those roots is drawn from the richest part of the vegetable earth; may not therefore the concoction thus formed, when both juices are in their purest state, and perfectly unmixed with other

other ingredients, complete that production of animated nature, which no other assemblage of matter could produce, and which is *concluded* and *finished* by a thin thread of the line of life, passing through each ball at its first formation? When aggregated into a larger mass, their circle was completed; and the thread which ties them all together is fixed never to be severed, but, passing with them through all their different habitations, in the side root, centre root, and alburnum vessel in the stem, *fixes them* at last in the seed-vessel, either incorporating the string with it, as in the lily, or hanging by it, as in the seed of the rose or violet, which seed is afterwards impregnated through this *identical string*. In the *cactus* tribe the balls being thoroughly divided, the string is admirably seen, being very thick in proportion, and so much more *woody* and *solid* than the *matter of the seed*, that it is easily distinguished. The seeds are found in every plant about six weeks or two months preceding flowering time, according to the season at which each plant performs that function. In fir trees they rarely begin to show their seeds till the seventh or eighth year of their age, and in other trees rather earlier.

That the embryo of trees is the same in all plants of the same genus, I am perfectly convinced, from the indifference in the procedure of the plant when grafted or budded; for the seeds mount as if nothing had happened, and pass into the bud. Indeed, the eye can mark little or no difference between the embryo of the herbaceous plant and that of trees; but the eye is *scarcely a fair judge* in so *delicate a matter*: probably this is the real cause that plants will not act in this way in any but their own genus. I have now shown the route this embryo of the seed takes before it is fixed in the bud during the time that part is in its cradle in the bark. I shall next endeavour to obviate, or rather examine into, the objections I first made myself, or others suggested to me, respecting this fact, and whether these balls should or not be believed to be the embryo of the seed.

When I first discovered them, I thought they might be air-bubbles: but they cannot be air; because, in all the *cactus* tribe, if the stem is cut in half in February, the seeds may be taken out with a pin, dried on a glass, then cut in half, and they will be found perfectly solid. Care must however be taken that a seed is selected *not covered with the pollen*, and that the *string* is *not broken too close* to the ball. It is covered with points when complete, and perfectly round. It cannot be air-bubbles, because, when collected in a heap at the bottom of the assemblage of buds before they pass into the dispersing vessel, to *view it well* the whole must be cut *longitudinally*, and divided into *two parts*; the heap is therefore of course separated, and many of the seeds *halved*,

halved, which plainly evinces *how solid they are*. They cannot be air; for, if placed in the air-pump, and a vacuum formed round them, it has not *the smallest effect* on them; whereas, place a real air-vessel, or rather a water plant with bubles of air in it, and the moment the vacuum is produced, all the bubbles *swell* and *burst*. But there are certainly no regular air-vessels, except in water or *some water plants*, though many air-bubbles accompany the sap. If they were not seeds, would they not appear at all times, and not be confined to the season *preceding the flowering of plants?* whereas they are rarely seen but at this time, except in those vegetables that flower twice, or for a long space of time. A few *strayed* on seed may, however, be found, but not in any quantity. Neither can they be *air surrounded* with the bark *juices* (as they sometimes appear in the firs), because they are really *solid*, and because in many trees they are so far removed from the *inner bark* vessels as to have no communication whatever with them. In the *sempervivum* tribe, the seeds are all in the alburnum vessels between two rinds, while the inner bark vessels, containing the bark juices, are *fixed* in the *interior between the second rind* and the wood, and cannot therefore approach the seeds. How admirably is this shown in a specimen of the plant! how plain and decisive is its effect on the mind! But drawings always leave a doubt behind. This situation of the alburnum is totally different from that of any other plant I am acquainted with, and only I believe to be found in the *sempervivums*.

I may be thought to have made too many objections, where the two first were all sufficient; but I might have added many more, to show how perfect the evidence is, since the vessel of impregnation passing through the seed, as it is always found in the flower, is a corroboration not a little powerful. It is *above four* seasons since I have pursued this discovery in *every mode* diligence *could suggest*; and I think I may now without fear present it to the public as an *absolute fact*. Malpighi gave a specimen of the alburnum vessel; but, not viewing it in the *proper season*, did not see *these balls:* he took however the intervening and smaller ones for *air-vessels*, (those which I suppose to be the powder of the pollen), and the middle vessels which contain the seeds (see fig. 1, Plate IV.) he called *absorbent vessels*, but designated *them no further*. But why should they not be the embryo of the seeds? We are now well assured that the *bulbous roots form* the flower in the root, and take in their seeds in the same place. If one set of plants can receive their seeds *there*, why should not all? It requires but cutting a hyacinth a month before blowing, and all its seeds will be seen mounting to the top of the root to enter the flower; but it must

be

be before the bud is *quite complete.* I forgot to mention that in most *firs* the bark juices are of a deep yellow brown, and the seeds of a most beautiful yellow white. All this evidence, strong as it is, is nothing to the perfect conviction the mind receives when in the arum the seeds are seen running up the middle flower-stem, and thus passing into each separate seed-vessel, or still plainer if possible in the larch, where a vessel is formed on purpose for transporting them to their proper situations. As these reasons are, I think, all sufficient, I will not trouble the reader with all I had prepared, but show, what is almost as plain, "that the pollen also is contained in that which forms and fills the intermediate spaces between the vessels which contain the seeds."

Malpighi took these vessels for air-vessels: but, *whatever they are*, after the seeds have deposited themselves into the seed-vessel, and that it has closed on them; *this powder*, which had before visibly run from the bark to the middle of the pith (now first receiving the coloured juices, which give them a yellow tint), *immediately proceeds* up the middle space *between the buds*, and enters them at the upper part instead of the under. In this specimen they cannot be seen to enter the pollen cases, but in the *arum*, they are seen to proceed *up* the *plant* and enter the *stamen* in the same manner as the seeds had done just before in their appropriate places. I have many reasons to suppose that the pollen powder is formed in the tap root; but this is not a fact of which I am so *perfectly assured* as of the preceding circumstance. I have certainly taken it from *thence*, but very unlike the seeds; the alteration of its form is so very great as to make it *much more difficult* to *trace*; it is most easy, however, to follow its progress as soon as it reaches the *alburnum vessels.* That the pollen receives its colouring liquor from the atmosphere, there can be no doubt; I have seen it enter the hairs, combine with the powder in the pith, dye a part of the pith with the same tint; and when the pollen is perfectly coloured, I have seen it run into the top of the bud, slowly proceeding to its destination. This is all to be traced point by point with a *single microscope.*

In taking a plant where the male and female flowers are separate, the balls pass to the female flower, the pollen to the male. In all the *firs* this is admirably *viewed* in the early part of the formation of the bud. In *real male trees* the balls never appear, and in female plants the pollen is never discovered; both seeds and pollen are often found in double flowers, where they never come to perfection; but here it is the impregnation of the seeds that fails; I conjecture that the impregnation is not required for the *forming* the *seeds*, or at least the *embryo of the seed*, but for its *nature* and *finish*; does it not here maintain

some

some resemblance in animal life? In the cypress it is beautiful to trace the different powders dividing and holding their course; the pollen to the male flower, the balls to the female. But still more astonishing is it, when, as in the *gooseberry* and *currant*, they are all pressed together, and yet never mistake their *destination*. In many plants, however, the pollen has a sort of funnel placed in the pith, through which the powder proceeds to its proper situation; but this is only just at the bottom of a collection of buds, and it soon disappears with the increasing growth of the plant.

As this short letter was only written to mention the time of the rising of the seeds, and to enable each botanist to judge of the discovery, I shall here *conclude*, hoping that I have obviated all the objections that can be made to its validity.

I am, sir,

Your obliged humble servant,

AGNES IBBETSON.

P. S.—I have troubled the reader with only two prints just to show the *specimen* of Malpighi, fig. 1. which is perfectly *just* (only he did not look at the proper time, and did not therefore see the seeds), and the seed-vessel of the *arum*, with the seeds mounting to the flower and entering the various seed-vessels: see fig. 2. The seeds running into the seed-vessels of the larch, I have already given in volume xxxv. page 1, of Nicholson's Journal.

I forgot to mention that in most fruits, *pears*, *apples*, *walnuts*, &c. the embryo of the seed is of a *greenish colour*.

XXXIV. *Letter from M.* AMPERE *to Count* BERTHOLLET, *on the Determination of the Proportions in which Bodies combine, according to the Number and respective Arrangement of the Molecules of which their integrant Particles are composed.*

[Continued from p. 116.]

WE have seen that eight tetrahedrons may be united in a representative form, which has been designated under the name of cubo-hexa-tetrahedron. In this arrangement, the position of two tetrahedrons differs from that of the six others; but it is easy to unite the same number of tetrahedrons, by giving to all of them the same respective position. For this purpose we shall conceive that one of the summits of each tetrahedron is placed at one of the eight solid angles of a cube, and that this tetrahedron is situated in such a way that its three other summits are in

in the plans which pass by the centre of the cube, and by the three sides of this cube which form the solid angle. The thirty-two summits of the eight tetrahedrons arranged in this way will be those of a polyhedron, which, in the case where the tetrahedrons are regular and have their centre of gravity at the same point, will have eighteen faces; viz. six square and twelve hexagonal.

It is easy to see that this polyhedron is nothing but a dodecahedron, of which the six summits with four faces shall have been curtailed by one-third of the adjoining ridges; as the position of the eight tetrahedrons of which it is composed is the same for all, I have given it the name of octo-tetrahedron. The eight tetrahedrons which form this polyhedron by their junction, are placed two and two like the two tetrahedrons which form a cube, and four and four like the four tetrahedrons of which the tetra-tetrahedron is composed; we may therefore consider it also as produced by the union of four cubes, or of two tetra-tetrahedrons.

The octo-tetrahedron having six faces, the middle parts of which are situated respectively like the six summits of an octohedron, we shall be able to unite these two polyhedrons into one only, in a manner analogous to that in which the combinations hitherto described are formed: but as this polyhedron is less simple than the amphihedron, which contains precisely as many tetrahedrons and octohedrons, and which consequently necessarily leads to the same results, relative to the combinations of bodies in determinate proportions; I shall not reckon it among the representative forms.

It is evident that the octo-tetrahedron, which has eight summits situated with respect to each other like the eight summits of a cube, cannot be combined with this form; but it may, like the hexa-tetrahedron, be combined with a hexahedral prism, because it partakes with the hexa-tetrahedron of the property of having hexagonal faces. In order to form a clear idea of this combination, we may conceive a line which joins the middles of the two opposite hexagonal faces of an octo-tetrahedron, and place it in a vertical situation; we then find that those two faces are each surrounded by six other faces, viz. two square and four hexagon; and that we may place a hexagonal prism in such a way as that, the six summits of each of its bases answering to those six faces, its axis will be confounded with the line situated vertically.

The two polyhedrons thus joined give a representative form, which differs only from the octo-tetrahedron inasmuch as the twelve faces of the latter, which surround the two bases, are covered by as many pyramids, four quadrangular and eight hexagonal.

agonal. As we cannot establish between the respective dimensions of the two polyhedrons, with the view of diminishing the number of faces, any relation which is symmetrical with respect to all the similar ridges, I shall suppose that they are such that the same sphere can be circumscribed for them; and the polyhedron with forty-four summits which results from this supposition, having seventy faces, viz. four hexagon, two square, and sixty-four triangles, I shall give it the name of eptaconta-hedron.

Finally, in order to combine the octo-tetrahedron with the tri-octohedron, it is sufficient to observe that each of these polyhedrons has as many summits as the other has faces, and reciprocally; we shall soon find that the positions of these summits and of these faces are precisely such, that by placing the six summits with four faces of the tri-octohedron on the perpendiculars raised in the midst of the six square faces of the octo-tetrahedron, all the summits of each polyhedron answer to the faces of the other.

If we determine the respective dimensions of the polyhedrons, so as that the ridges of the tri-octohedron which join at the six summits just mentioned may pass by the middles of the ridges of the square faces of the octo-tetrahedron, there will result from the junction of those two representative forms a new polyhedron which will have fifty summits and seventy-two faces; viz. twenty-four quadrilateral and forty-eight isosceles triangles. This polyhedron may be considered as a tri-octohedron, of which the thirty-two faces shall have been covered by as many triangular pyramids: this is the reason why I designate it by the name of pyramidated tri-octohedron.

I shall only point out three other representative forms, composed of four, five, and seven octohedrons, and to which I have given the names of tetra-octohedron, penta-octohedron, and epta-octohedron; and for the sake of brevity, I shall not speak of the combinations which may be made of those three representative forms with the preceding polyhedrons.

If we take notice that, one octohedron being given, there are four different positions in which another octohedron of the same size forms with the first a hexahedral prism, we shall easily conceive that four octohedrons situated in those four positions will have their centre of gravity at the same point, and will form a combination into which they will all enter in the same way. This combination is the tetra-octohedron, which has twenty-four summits and fourteen faces, of which six are octagon and the eight others are equilateral triangles: by adding thereto the same octohedron which has served to determine the respective positions of the four octohedrons which we have combined, we shall have the penta-octohedron, the summits of which are thirty in

in number, and which has fifty-six triangular faces, of which eight are equilateral and the other forty-eight are isosceles.

If, instead of uniting the tetra-octohedron with a single octohedron, we combine it with a tri-octohedron, by placing the six summits with four faces of the latter at the same point where we have placed the six summits of the fifth octohedron in the formation of the penta-octohedron, we shall have the polyhedron composed of seven equal octohedrons, to which I have given the name of epta-octohedron, and which has forty-two summits and eighty triangular faces, of which eight are equilateral, twenty-four isosceles, and forty-eight scalene.

I subjoin a comparative table of these twenty-three representative forms.

	Number of Tetrahedrons.	Number of Octohedrons	Number of Summits.	Number of Faces. Triangles.	Quadrilateral.	Hexagonal.	Octagonal.	Total of Faces.
Tetrahedron (1)	1	0	4	4	0	0	0	4
Octohedron	0	1	6	8	0	0	0	8
Parallelopipedon	2	0	8	0	6	0	0	6
Prism hexahedron	0	2	12	0	6	2	0	8
Dodecahedron	2	1	14	0	12	0	0	12
Hexa-decahedron	1	1	10	16	0	0	0	16
Triacontahedron	2	2	20	24	6	0	0	30
Trioctohedron	0	3	18	32	0	0	0	32
Trapezoidal	2	3	26	0	24	0	0	24
Tetra-hedron	4	0	16	28	0	0	0	28
Penta-tetrahedron	5	0	20	12	12	0	0	24
Hexa-tetrahedron	6	0	24	0	6	8	0	14
Hexa-tetrahedron pyramidated	6	1	30	24	0	8	0	32
Cubo-hexa-tetrahedron . .	8	0	32	48	6	0	0	54
Amphihedron	8	1	38	24	24	0	0	48
Pentacontahedron	6	2	36	36	12	2	0	50
Octocontahedron	6	3	42	80	0	0	0	80
Octo-tetrahedron	8	0	32	0	6	12	0	18
Eptacontahedron	8	2	44	64	2	4	0	70
Trioctohedron pyramidated .	8	3	50	48	24	0	0	72
Tetra-octohedron	0	4	24	8	0	0	6	14
Penta-octohedron	0	5	30	56	0	0	0	56
Epta-octohedron	0	7	42	80	0	0	0	80

It is by these polyhedrons that I have represented the various arrangements of the molecules of all bodies. When these bodies contain such substances only of which we may measure the volume in the state of gas, we have immediately the number of the molecules of each species which enter into their composition. When a simple body cannot be obtained in the state of gas, we must try in succession various suppositions relative to the number of molecules of this simple body, which are contained in one of the compounds which it forms with a gaseous substance, oxygen for instance. The relations in weight make known the number of the molecules of the same body which enter into its other compounds; and the condition which it must satisfy, that all the numbers of molecules which are obtained correspond to polyhedrons comprehended in the foregoing table, soon makes known that one of these different suppositions which can agree with the entire whole of the phænomena: it then becomes easy to calculate the respective weights of the molecules of all the simple bodies; and these weights once determined, it is sufficient to have a pretty close analysis of a compound body, and to know how much its particles contain of molecules of each of its elements, and thus correct the inevitable errors of analysis.

Several chemists have endeavoured to attain the same result by determining the respective weights of certain proportions of the different simple bodies which always enter an entire number of times into the bodies which are composed of them. These proportions do not lead to results conformable to the experiments, but when they are always multiples or submultiples of the respective weights of the molecules: but when we make use of them, nothing can indicate how many of the proportions of a simple body ought to enter into one of those compounds; whereas the consideration of the representative forms shows, in many cases, how much in a compound body there ought to enter of molecules of each of its elements, and even leads us to establish between the combinations of two simple bodies with all the others, such a dependence, that, the combination of one of those bodies being known, we may foresee those of the other. I have found for instance, by comparing the combination which oxygen and hydrogen form with different bodies, that, with the exception of chlore and sulphur, the combinations of which with hydrogen present the properties of the acids, one same quantity of a body susceptible of being united to hydrogen is combined in such a manner, that there are in general, in each of the particles of the compound, four molecules of hydrogen more than there are molecules of oxygen in the corresponding combination of the same body with this last gas. We may even remark, that when this body forms with oxygen several combinations, some of which are more difficult

difficult and the others more easy to decompose, the compounds of hydrogen corresponding with the first in the manner which I have explained, are the only ones which can be obtained, at least in the isolated state; and that those which correspond in the same manner with the less stable combinations of oxygen are either impossible, or cannot exist but when united to a third body. According to this addition of four molecules or of an entire particle of hydrogen to the number of the molecules of oxygen assumed by the different bodies in their most stable combinations with this last gas, we find six molecules of hydrogen when there are two of oxygen in these combinations, and eight when there are four.

The same considerations also lead us to foresee, according to the representative forms of their particles, what are the gases which water cannot absorb, but in very small quantity, by the simple interposition of some of their particles between those of water, and those which the same liquid is susceptible of absorbing in large quantities, and forming with them true combinations.

[To be continued.]

XXXV. *On Mr.* Fearne's *Observations on external Perception, &c.* *By* T. Forster, *Esq.*

To Mr. Tilloch.

Sir, In a paper On external Perception, recently published in the Pamphleteer, by Mr. Fearne, he has maintained an opinion that our perception of external objects is not by means of the five senses alone, but that it is an act of the mind. As this doctrine, at least as expressed in the manner he has delivered it, is somewhat novel; and as it coincides with an opinion which has resulted from the last discoveries into the physiology and structure of the organs of the brain, I am induced to point out to the metaphysical speculator the great similarity of opinions drawn from different sources, on a subject which has so frequently engaged the attention of philosophers. I do not proceed with Mr. Fearne in all his reasonings throughout the course of his various metaphysical inquiries, but allude specially to the opinion that the sensation of objects by means of the five external senses is not sufficient to produce the belief of the external existence of bodies. But I believe the perception of the external bodies to be the consequence of an organic apparatus quite as material as the nerves of the five senses. In short, the organ of individuality, or that part of the front lobes of the brain above the nose, in the middle and inferior part of the forehead, is

the cause of our belief of external bodies. This organ also excites the activity of the five senses, and thereby is active, by means of them, in producing the sensation of bodies, which it furthermore causes us to regard as external, individual, and existent, independent of our sensations of them. I have long conceived, as I formerly mentioned, that there must exist some special faculty in the mind for this perception of individuality; but it was Dr. Spurzheim who pointed out the seat of its organ. I recommend the study, therefore, of the structure and organic arrangement of the brain published by him and Gall, to all those who wish to see the material engines of the various functions of the mind. They will be really surprised, if they follow up this course of anatomical and physiological studies attentively, at the great progress which has in so short a time been made in a science which has for ages been regarded as the most difficult and at the same time the most interesting in the world. The phrenology of Gall and Spurzheim has verified what Richerand hinted at as the *ne plus ultra* of physiology, when he anticipated a mode to solve the problem of the principles of the actions of animated beings; and, given the physical structure of any person, to find his moral and intellectual qualities.

The proofs that the aforesaid faculty of individuality resides in the particular organ ascribed to it, are founded on a series of facts too numerous to be detailed here. I have fortunately had many cases both of the great as well as of the defective development of that organ, to confirm and put beyond all doubt in my mind the truth of the physiology he has imputed to it. And I mention this apparent coincidence of opinion, to excite others to examine; as these very abstruse considerations, such as are contained in Mr. Fearne's work, are very liable to be misunderstood, and it becomes desirable to call forth and collect the different views of the philosophy of mind which they excite in people of different organizations.

Yours, &c.

Cambridge, March 11, 1815. THOMAS FORSTER.

XXXVI. *On the Pyramids of Egypt. Extracted from Dr.* EDWARD DANIEL CLARKE'S *Travels*, Vol. III.*

PYRAMIDS OF DJIZA.

As we drew near the base of the principal pyramid, the effect of its prodigious magnitude, and the amusement caused in view-

* Dr. Clarke's account of the pyramids is by far the most philosophical and satisfactory of any that have been offered to the world on these interesting monuments of antiquity. Our readers, we are confident, will be highly gratified to find it in our pages.

ing

ing the enormous masses used in its construction, affected every one of us; but it was an impression of awe and fear rather than of pleasure. We had heard the pyramids described as huge objects which gave no satisfaction to the spectator, on account of their barbarous shape, and formal appearance; yet to us it appeared hardly possible, that persons susceptible of any feeling of sublimity could behold them unmoved. With what amazement did we survey the vast surface that was presented to us when we arrived at this stupendous monument, which seemed to reach the clouds! Here and there appeared some Arab guides upon the immense masses about us, like so many pygmies, waiting to show the way up to the summit. . . . . .

The mode of ascent has been frequently described; and yet, from the questions that are often proposed to travellers, it does not appear to be generally understood. The reader may imagine himself to be upon a staircase, every step of which, to a man of middle stature, is nearly breast high; and the breadth of each step is equal to its height: consequently the footing is secure. Where the stones are decayed, caution may be required; but upon the whole the means of ascent are such that almost every one may accomplish it. . . . . .

At length we reached the topmost tier, to the great delight and satisfaction of all the party. Here we found a platform thirty-two feet square; consisting of nine large stones,—though much inferior in size to some used in the construction of this pyramid. . . . .

The view from this eminence amply fulfilled our expectations; nor do the accounts which have been given of it, as it appears at this season of the year [August 23d], exaggerate the novelty and grandeur of the sight. All the region towards Caïro and the Delta resembled a sea, covered with innumerable islands. Forests of palm-trees were seen standing in the water; the inundation spreading over the land where they stood, so as to give them an appearance of growing in the flood. To the north, nothing could be discerned, but a watery surface thus diversified by plantations and by villages. To the south we saw the pyramids of Saccára; and, upon the east of these, smaller monuments of the same kind, nearer to the Nile. An appearance of ruins might indeed be traced the whole way from the pyramids of Djiza to those of Saccára; as if they had been once connected, so as to constitute one vast cemetery. Beyond the pyramids of Saccára, we could perceive the distant mountains of the Saïd; and upon an eminence near the Libyan side of the Nile appeared a monastery of considerable size. Towards the west and SW the eye ranged over the great Libyan Desert, extending to the utmost verge of the horizon, without a single object to interrupt

the dreary horror of the landscape, except dark floating spots, caused by the shadows of passing clouds upon the sand.

Upon the SE side is the gigantic statue of the Sphynx, the most colossal piece of sculpture which remains of all the works executed by the ancients. The French have uncovered all the pedestal of this statue......it proves to be a wretched substructure of brick-work and small pieces of stone, put together like the most insignificant piece of modern masonry*.....Beyond the sphynx we distinctly discerned, amidst the sandy waste, the remains and vestiges of a magnificent building (unnoticed by former authors who have written upon the pyramids)—perhaps the SERAPEUM.......

Immediately beneath our view, upon the eastern and western side, we saw so many tombs that we were unable to count them, some half buried in the sand, others rising considerably above it. All these are of an oblong form, with sides sloping like the roofs of European houses. A plan of their situation and appearance is given in Pococke's Travels. The second pyramid, standing to the SW, has the remains of a covering near its vertex, as of a plating of stone which had once invested all its four sides. Some persons, deceived by the external hue of this covering, have believed it to be of marble; but its white appearance is owing to a partial decomposition, affecting the surface only. Not a single fragment of marble can be found near this pyramid. It is surrounded by a paved court, having walls on the outside, and places as for doors or portals in the walls; also an advanced work or portico. A third pyramid, of much smaller dimensions than the second, appears beyond the sphynx to the SW; and there are three others, one of which is nearly buried in sand, between the large pyramid and this statue, to the SE.

Having thus surveyed the principal objects, as they appeared from the summit of the great pyramid, we proceed to the examination of the substances which compose its exterior surface.

* The author afterwards notices, that as they drew "near to view this prodigious colossus, a reddish hue was discernible over the whole mass, quite inconsistent with the common colour of the limestone used in building the pyramids, and of which the sphynx is formed." This induced further examination; and "having succeeded in climbing beneath the right ear, where the surface had never been broken, nor in any degree decomposed by the action of the atmosphere," Dr. Clarke found that the whole had once been painted of a dingy red or blood colour; and on this painted surface he found an inscription, but so concealed under the enormous ear of the sphynx that no notice has been taken of it by any preceding traveller. The two first lines are Coptic; the rest is Arabic; and the characters, which are of considerable size, are black painted on the red surface.

The

The stones of the platform upon the top, as well as most of the others used in constructing the decreasing ranges from the base upwards, are of soft limestone; a little harder, and more compact, than what some of the English masons vulgarly call *clunch;* whereof King's College Chapel at Cambridge, and great part of Ely Cathedral, are built. It is of a grayish-white colour, and has this remarkable property, that, when broken by a smart blow with a hammer, it exhales the fetid odour common to the dark limestone of the Dead sea, and of many other places.

......It is now very generally admitted, that the stones are of the same nature as the calcareous rock whereon they stand, and that this was cut away in order to form them.....As we descended from the summit, we found mortar in all the seams of the different layers upon the outside of the pyramid; but no such appearance could be discerned in the more perfect masonry of the interior. The mortar is of a coarse kind, and contains minute fragments of *terra cotta*.....

The French had been very assiduous in their researches among these buildings. They even attempted to open the smallest of the three pyramids; and, having effected a very considerable chasm in one of its sides, have left this mark behind them, as an everlasting testimony of their curiosity and zeal. The landing of our army put a stop to their labour; but for this, the interior of that mysterious monument would probably be now submitted to the inquiry which has long been an object among literary men.

......Having collected our party upon a sort of platform before the entrance of the passage leading to the interior, and lighted a number of tapers, we all descended into its dark mouth. The impression made upon every one of us, in viewing the entrance, was this; that no set of men whatever could have thus opened a passage, by uncovering precisely the part of the pyramid where the entrance was concealed, unless they had been previously acquainted with its situation; and for these reasons: first, because its position is almost in the centre of one of its planes, instead of being at the base. Secondly, that not a trace appears of those dilapidations which must have been the result of any random search for a passage to the interior; such as now distinguish the labours of the French upon the smaller pyramid, which they attempted to open. The persons who undertook the work, actually opened the pyramid in the only point, over all its vast surface, where, from the appearance of the stones inclined to each other above the mouth of the passage, any admission to the interior seems to have been originally intended.......Strabo describes not only the exact position of the mouth of the pyramid, but even the nature of the passage leading to the Θήκη, or

 Soros,

Soros, in such a manner, that it is impossible to obtain, in fewer words, a more accurate description*. It seems also true, that this opening had been made before the time of Herodotus, although his testimony be less decisive. He speaks only of subterraneous chambers †; but it were impossible to know any thing of their existence, unless the pyramid had first been entered.

Hence it is evident that a passage to the interior had been open from the earliest times in which any account was given of this pyramid; and perhaps it never was so completely closed, but that with a little difficulty an access might be effected. Proceeding down this passage [minutely described by Greaves], and ascending a second sloping channel to the distance of 110 feet, we came to a horizontal passage, leading to a chamber with an angular roof. In this passage we found on our right hand the mysterious well, which has been so often mentioned. [Pliny makes the depth 129 feet—Greaves and others 20 feet; but the latter were deceived by the plummet resting on some projection.] On getting a large stone, almost as wide as the mouth of the well, we were agreeably surprised by hearing, after some seconds, a loud and distinct report, seeming to come from a spacious subterraneous apartment, accompanied by a splashing noise, as if the stone had been broken into a number of pieces, and had fallen into a reservoir of water at an amazing depth......

We examined the chamber at the end of this passage, mentioned by all who have described the interior of this building. Its roof is angular; that is to say, it is formed by the inclination of large masses of stone leaning towards each other. Quitting the passage altogether, we climbed the slippery and difficult ascent which leads to the principal chamber. The workmanship, from its perfection, and its immense proportions, is truly astonishing. All about the spectator, as he proceeds, is full of majesty, and mystery, and wonder.....Presently we entered that "glorious room," as it is justly called by Greaves, where, "as within some consecrated oratory, Art may seem to have contended with Nature." It stands "in the very heart and centre of the pyramid, equidistant from all its sides, and almost in the midst between the basis and the top." The floor, the sides and roof are composed of that most beautiful variety of *granite* which Linnæus distinguished by the epithet of *durus rubescens*, called by the Italians *granito rosso*, composed essentially of feldspar, quartz, and mica.....It differs in no respect from European granite, except that the red feldspar enters more largely as a constituent into the mass than is usual in the granite of Europe. So exquisitely

* *Strab. Geog.* lib. xvii. p. 1145. Ed. Oxon.

† *Herodot.* Euterpe, c. 125.

quisitely are the masses of this granite fitted to each other upon the sides of this chamber, that, having no cement between them, it is impossible to force the blade of a knife between the joints. We tried the experiment. There are only six ranges of stone from the floor to the roof, which is twenty feet high; and the length of the chamber is about twelve yards. It is about six yards wide. The roof or ceiling consists only of nine pieces, of stupendous size and length, traversing the room from side to side, and lying like enormous beams across the top.

Near the western side stands the *Soros*, of the same kind of granite as that which is used for the walls of the chamber, and as exquisitely polished. It is distinguished by no difference of form or dimensions from the common appearance of the Soros, as it is often seen in Turkish towns when employed by the inhabitants to supply the place of a cistern. It resembles, as Greaves has remarked, "two cubes finely set together, and hollowed within; being cut smooth and plain," without sculpture or engraving of any kind. Its length on the outside is seven feet $3\frac{1}{2}$ inches; its depth three feet $3\frac{1}{4}$ inches; and it is the same in breadth. Its position is north and south.

This beautiful relique was entire when our troops were landed in Egypt . . . .but our soldiers and sailors, with the view of carrying pieces as curiosities to England, began, alas! the havoc of its demolition; and but for the classical taste and laudable interference of Colonel now General Stewart, not a particle of the Soros would have remained. The persons who thus left behind them a sad memorial of the British name, had only succeeded in accomplishing a fracture near one of the angles. Every traveller of taste will join in reprobating any future attempt to increase the injury it has so lamentably sustained.

PYRAMIDS OF SACCARA.

Just beyond Menshee Dashoo we were much struck by the appearance of a *tumulus* (standing to the south of a large graduated pyramid), which, instead of being pyramidal, exhibits a less artificial and therefore a more ancient form of sepulchre than any of the pyramids. It is a simple hemispherical mound. We saw afterwards others of the same kind. . . . . . .

These pyramids appear to be a continuation of the great cemetery to which those of Djiza also belonged. They extend four or five miles, both to the north and to the south of the village of Saccára. Some of them are rounded at the top, and, as it was observed by Pococke, "do not look like pyramids, but more like hillocks cased with stone." One of these is graduated like the principal pyramid at Djiza; but with this difference, that the gradations here are much larger, although the pyramid

be smaller. It consists only of six tiers or ranges of stone; the pyramid itself being 150 feet in height. The ranges of steps are twenty-five feet high and eleven feet wide. The rest of these structures are so fully and accurately described by Pococke, that little will be added here to his description of them. There is one built also with steps, which he believed to be as large as the principal pyramid at Djiza. The works at Saccára, independently of the different forms which characterize them, do all appear to be older than those of Djiza; the buildings being more decayed, and the stones crumbling, as if decomposed by longer exposure to the action of the atmosphere. Four miles to the south of Saccára stands a pyramid built of unburned bricks. This is in a very mouldering state. The bricks contain shells, gravel, and *chopped straw*; they are of the same nature as the unburned bricks in modern use in Egypt. Pococke concluded, from its present appearance, that it was built with five gradations only. It is of the same height as the other graduated pyramid of six degrees*.

[To be continued.]

XXXVII. *Observations on a Paper by* G. A. De Luc, *Esq. containing some Remarks on Mr.* Donovan's *Reflections concerning the Inadequacy of electrical Hypotheses. By* M. Donovan, *Esq.*

To Mr. Tilloch.

Sir,—In your last are contained some Observations on my "Reflections on the Inadequacy of the principal Hypotheses to explain the Phænomena of Electricity," by Mr. De Luc. Any thing from the pen of that venerable and distinguished philosopher is deserving of the highest consideration: you will therefore allow a place to a few observations in reply.

It may in the first place be necessary to apprise Mr. De Luc of a misconception into which he has been led, no doubt, by some defect in the statement of my objections to the Franklinian hypothesis.

The hypothesis of electric excitation, in which it is supposed that, during the attrition of glass, the pores by opening receive electricity, and expel it when they close, is attributed by Mr. De Luc to me. To this, however, I have no title; it is a statement

* The author afterwards examined the catacombs here and at Alexandria; and he has furnished abundant evidence, from the position of the *Soroi* found in the latter, and from the height of the space in which the mummies were deposited, that in these cemeteries they were invariably placed in a horizontal position—not upright as maintained by some authors.

ment taken from Franklin, and will be found in his "Experiments and Observations on Electricity, London 1769," p. 78. The experiment detailed by Mr. De Luc, which, as he observes, "is a peremptory demonstration that the effect of friction is not to open the pores for receiving more electric matter, which is discharged when the friction ceases, as Mr. Donovan conceives it," therefore, does not prove against me, but against Franklin.

Mr. De Luc declares that "an error of Franklin has created all the just objections of Mr. Donovan." They are then admitted to be well founded, and my object is accomplished. It was Franklin, and not Volta, whose opinions I combated; and therefore Mr. De Luc's observations all relate to the possibility of applying to Volta what was intended for Franklin; so that, between the opinions of Mr. De Luc and those which I have ventured to being forward, there is as yet no disagreement.

That I had no opportunity of an acquaintance with the *Idées sur la Météorologie* and the *Traité élémentaire* of Mr. De Luc is true; and I regret that works such as I suppose them to be never came within my reach. Living in a city where literary advantages of this kind are far less numerous than in other places, I have had often to feel and lament the consequent limitation of my knowledge on subjects with which I was most anxious to be acquainted.

Of the hypothesis of Volta concerning the standard of electrical comparison I was not altogether ignorant; indeed the only knowledge I possessed on the subject was derived from some papers of Mr. De Luc's in Nicholson's Journal. I formerly searched into various books with the design of gaining a better acquaintance with Volta's view of this standard, but I was unable to obtain it. I found, it is true, a paper by Volta in some of the old Philosophical Transactions, written in Italian: it related to the action of electric atmospheres upon his condenser, but they did not afford what I desired.

To enter into a discussion with so experienced a philosopher as Mr. De Luc, were to presume myself what I am conscious of not being. I therefore decline that task, and content myself with barely submitting to him a few suggestions concerning his defence of Volta, and his observations on my objections.

Mr. De Luc by a striking experiment has illustrated the Voltaic hypothesis. By the continued dispersion of electricity in a room, the air was rendered positive. A pair of insulated pith balls in the natural state were brought in from an adjoining room: they now diverged negatively; but when returned to the room from whence they came, the natural state was resumed. Mr. De Luc explains this by supposing that, although the balls were at first in the natural state when compared with the electric state of that room,

room, yet, when brought into an air containing a greater *absolute* quantity of electricity, they became relatively negative. Now, if this admit of a different explanation, the necessity of the above inference is destroyed: it is therefore of importance to try if this can be effected.

With regard to my experiment in which bodies possessed of similar states attract each other; Mr. De Luc considers that the excited electric repelled the similar electricity of the pith ball through the thread into some other part: the effect is owing to what Volta calls electric influence, and it appears to constitute an essential part of his system. But with this I have been long familiar; not as belonging to Volta, but to Franklin. Now it appears to me, that this principle of electric influences may be applied in explanation of Mr. De Luc's experiment on electrified air. The balls in the natural state are brought into a positive atmosphere; the electricity of the latter repels the natural quantity of the balls into their internal substance: the external parts are therefore left minus. With this view, the sequel exactly corresponds; for, when the balls were brought back into the unelectrified room, they collapsed into their natural state. But, on the opposite view, the balls by immersion into a positive atmosphere should assume that state, and retain it when removed, and therefore still continue to diverge. Hence it would appear to me that the experiment is an anomaly on the hypothesis of Volta, and perfectly reconcileable to the principles of Franklin.

But my experiment above referred to, in which bodies of a similar state attracted each other, does not in my mind appear to be explained by the principle of electric influences. Mr. De Luc supposes that the excited tube repels the electricity of the ball through its thread, leaving it ultimately negative, and therefore attracting it. But at the moment when this attraction is about to take place, if the tube be dexterously removed, it will be found that the ball is still *positive*.

Furthermore, it is for Mr. De Luc's consideration, whether the principle of electric influences be even applicable to this experiment. Volta's principle, if I understand it, relates to two bodies, one of which is in the natural state, and the other in a supernatural state. In my experiment they are both in a supernatural state.

WIth regard to the explanation of the motion of electrified bodies given by Volta, and cited by Mr. De Luc, p. 99, I need only observe that it virtually destroys the principle of repulsion, and refers all to attraction. How then can it be at the same time supposed that electricity is an elastic fluid?

The experiment which I consider subversive of Franklin's supposed impermeability of glass, appears to Mr. De Luc explicable

by

by the hypothesis of Volta. This philosopher analyses the electric fluid into two elements: the one, called electric matter, cannot pass through the glass; the other, called deferent fluid, passes with ease. Mr. De Luc subjoins: "so far this system might appear only an hypothesis to Mr. Donovan, as he does not know the work to which I refer." I must confess that to me the whole has a very hypothetical appearance, but I am willing to admit my ignorance of the above-mentioned work as the cause. There is not, however, any hypothesis in the simple experiment which was detailed in my former paper. A thin flask with a neck many inches long was half filled with mercury, and coated on the outside to the same height with foil. By means of a moveable wire, an electric charge was thrown in; the wire was drawn out, and the neck was hermetically sealed. After a length of time the sealing of the neck was cut off; the wire was plunged into the mercury; but not the slightest commotion was perceived by the hand, although the original charge was capable of giving a violent shock. Thus I had encompassed a quantity of electricity on all sides by glass; after a certain time I found that the glass contained none:—What should I conclude, but that it escaped? Were Mr. De Luc to repeat this experiment, he would certainly consider my inference natural.

Many more observations might be made, but these are all that I consider necessary to offer for Mr. De Luc's consideration. The object of my former paper was not to defend any hypothesis, but, on the contrary, to show that we are deceived when we suppose ourselves possessed of any knowledge of the electric fluid beyond the perception of its sensible effects. The only mode of inviting truth is to remove the barriers which oppose its progress. I am persuaded that there would be no one more willing than Mr. De Luc to lend his assistance to the acquirement of so desirable an object.

I am, sir, &c. &c.

Dublin, March 10, 1815. M. DONOVAN.

XXXVIII. *On certain Products obtained in the Distillation of Wood, with some Account of bituminous Substances, and Remarks on Coal.* By J. MACCULLOCH, *M.D. F.L.S. Chemist to the Ordnance, and Lecturer on Chemistry at the Royal Military Academy at Woolwich**.

It is well known that when wood and other vegetable substances are submitted to destructive distillation, there is produced, among other matters, a black dense fluid resembling molasses or com-

* From the Geological Transactions, vol. ii.

mon

mon tar. My connection with the Ordnance powder-mills having compelled me to examine some of the properties of this substance, which is obtained in large quantities in the proces of making charcoal, I was induced to extend my inquiries, in consequence of finding that the nature of the compound has not hitherto been understood.

As it is commonly called *tar* by the workmen, I shall use this term for want of a better.

This tar is very inflammable, and so liquid that it may be burnt in a lamp.

Although it appears to be an uniform fluid, it contains a great quantity of acetic acid, in a state of loose combination or mixture. For, by washing with water, a great part of this is separated; the water at the same time acquiring a colour from a portion of the tar which is retained in solution by the acid. Boiling water takes up a larger portion, and the tar acquires from this operation a thicker and more pitchy consistence.

Lime and the carbonated alkalies separate the acid with ease, carrying away also a portion of the tar which continues united to the solution. With subcarbonate of potash it thus forms in the first instance an uniform solution of a brown colour; but a continuance of trituration or boiling renders it pitchy and tenacious, after which it forms no further union with the mild alkalies.

It is perfectly and readily soluble in alcohol, in ether, in the pure alkaline lixivia, in acetic acid, and in the mineral acids. The fat oils and the new essential oils dissolve only a small portion of it; but the drying oils and the latter when thickened by age act more readily. Coloured oil of turpentine dissolves a good deal of it. Naphtha hardly exerts any action, acquiring a scarcely sensible brown colour. If heat be applied to assist the solution, the portion taken up is again deposited on cooling.

When it is subjected to distillation in a heat sufficient to keep it in a gentle ebullition, an oily-looking matter passes over in considerable proportion, which sinks to the bottom of the water into which the tube is inserted. It is first of a pale colour, resembling oil of peppermint, but becomes gradually darker as the operation advances, till it acquires a deep brown hue.

If the operation be pushed by increasing the heat of the retort to redness, there remains at length only a mass of spungy charcoal, and the substance is totally converted into the following new compounds, namely, the residuary charcoal, the oily matter, and the matter held in solution in the water of the apparatus. This latter proves to consist of a large portion of acetic acid, with which is combined a very little ammonia.

There is no inflammable gas given out in this process unless the

the heat be carelessly managed. If the vapour of the oily matter as it arises be exposed to the sides of the retort elevated to a high temperature, it is decomposed; and instead of oil there are thus obtained by a violent distillation in a naked fire, scarcely any products but acetic acid and an inflammable gas. This fact is analogous to those occurring in the ordinary process for decomposing such inflammable bodies as can be made to put on the gaseous state—and we ought, in fact, to consider every process of this kind, where a rapid distillation with a hot fire is used, as a succession of decompositions; the matter first produced being afterwards exposed to another process of destruction. It is not therefore perhaps very correct language, to say that vegetables yield a great quantity of inflammable gas on distillation with a naked fire: this is the produce of a second distillation, which by the common mode of operating is confounded with the first. As this reasoning applies equally to all other similar processes, it would be desirable to use a more accurate mode of describing this common operation, by which we might in some important instances be led to a more correct practice. Thus, for example, in the common mode of distilling coal to produce the inflammable gases, this double operation is carried on at once by the application of the petroleum and naphtha at first produced to the heated iron of the retort. It is in consequence of this imperfect mode of exposing the fluids thus generated to a second heat, that so large a portion of the petroleum is distilled unchanged. By causing it to pass a second time in contact with heated iron, while in the state of vapour, it may be resolved completely into inflammable gas and charcoal, and the produce of gas be thus considerably increased. This circumstance explains also the contradictory accounts given by different persons of the relative products of distillation, as applied to the various compound inflammables. To instance the case of camphor, which, according to the mode of managing the process, may be caused to yield essential oil, or inflammable gas, or a mixture of both in various proportions: I need scarcely point out the advantages so obviously to be derived from this consideration in the œconomical process of procuring light from pit-coal, an operation at present conducted with less skill than it demands.

I distilled a portion of this tar in such a way as to obtain inflammable air only, and took the gas in five portions. The first burnt very faintly, the second rather better, the third and fourth portions with a good white flame, and the fifth burnt feeble and blue. No portion of it was equal in brilliancy of inflammation to the gas from pit-coal. On examination, it was found to contain much carbonic oxide, by which its nature, as far as it differs from the gas of coal, is readily understood. The cause of this difference

difference will be apparent when the other circumstances in the constitution of this substance have been detailed. I thought it superfluous to examine accurately the nature of these gases; but they probably consist of different mixtures of carbonic oxide, with light and heavy hydrocarbonate and olefiant gases, if indeed (as I much doubt) there be any real boundary by which the composition of these three last gases can be defined.

If the process of distillation which I have now described be stopped when the oily matter begins to acquire a brown colour, and when the production of acetic acid is less perceptible, the matter in the retort will be found, when cold, to have assumed a solid consistence. In this state it resembles either pitch or asphaltum, according to the degree of heat it has undergone after it became capable of solidifying.

I will describe this substance as it appears when it first becomes solid, the reason of which will soon be apparent.

Previously to its arriving at this state, it bears a considerable resemblance to maltha, being of a consistence intermediate between that of petroleum and asphaltum: but I did not completely examine its chemical properties in this condition, because they appeared not to differ from what might be expected, and its history will be sufficiently full without it. In the solid state it is brilliant and shining, and breaks with a conchoidal fracture and some external resemblance to obsidian. It has a pungent burning taste, and the well known smell of wood smoke. It is heavier than the specimen of asphaltum with which I compared it, having a specific gravity of 1·254, while that of the asphaltum was 1·202. It is fusible and readily inflammable, burning with a white flame. It is electric, and exhibits the same electricity as the resinous bodies. When heated in an open vessel, it smokes; and if kept in fusion till it ceases to smoke, it at length ceases to be fusible, and is ultimately converted into a coal. During this progress it becomes more brilliant and less fusible, its fracture also from conchoidal becomes more splintery, and it puts on the appearance of asphaltum so accurately that the eye cannot detect the difference. Its specific gravity also diminishes, and its chemical properties vary in the way I am now about to detail.

I have described the perfect solubility of the tar in alcohol. The softest specimens of the pitch are nearly as soluble, leaving only a small residuum, which is infusible and powdery. The harder specimens become in proportion less soluble, and leave a larger residuum; and those which have been the longest exposed to heat scarcely give a stain to the alcohol, resembling in this respect the driest specimens of asphaltum. The analogy is here very apparent; for asphaltum may approach more or less to petroleum, and the various specimens of it are found to exhibit various

various degrees of solubility in alcohol. That which is least fusible in the fire, is, in both cases, the least soluble in alcohol. And by this consideration, the jarring accounts which have been given of the solubility of asphaltum in alcohol may be reconciled; and it will be seen in the sequel, that the history of this substance illustrates, in every respect, the true nature of the several varieties of the bitumens, substances whose mutual relations, and the causes of whose chemical diversity, have hitherto not been understood.

If a perfectly soluble specimen be dissolved in alcohol, it is obtained unchanged by evaporating the spirit. In any other case, the matter which the alcohol has taken up is precisely similar to the pitch in its first state, and the residuum resembles that which is the result of fusion when it refuses longer to melt. Alcohol therefore separates the pure pitch from that which by a process of decomposition has been nearly carbonized. Ether acts upon this substance as readily and in the same manner as alcohol does. In lixivium of pure potash it is more completely soluble than in alcohol, and forms with it an intensely brown solution, which is diffusible in water without change, and which, on the addition of an acid, deposits the matter in a powdery form and apparently unchanged. It is also soluble in water of ammonia with similar appearances. It is scarcely soluble in the pale oil of turpentine, but more readily in the darker. It is slightly soluble in the fat oils, in tallow and in wax, but is considerably more soluble in drying oil. In all these cases its solubility varies, from the same causes as those which affect its solubility in alcohol. Naphtha, whether pale or brown, has no action on it when cold, and takes up but a very minute proportion even with the assistance of heat. It fuses into an uniform mass with sulphur, with resin, and with asphaltum.

Acetic acid, which dissolves so many of the compound inflammables, effects a complete solution of it and in large proportion, and this compound is precisely similar to the empyreumatic acid as it proceeds from the iron retorts in which the charcoal is distilled. It would be desirable, in an œconomical point of view, to discover a method of freeing the acid from the pitch. After many trials, by combining the foul acid with various bases and again separating it, it was always found to retain the overpowering smell of wood tar. If the acid is combined with the pitch at a high temperature, a large proportion of it separates in the form of tar on cooling. Muriatic acid, after long boiling on the pitch, became brown, and dissolved a little of it.

By digestion with sulphuric acid it was dissolved, forming a brown oily-looking fluid, sulphureous acid being at the same time disengaged. By dilution with water, a smell resembling pepper-mint

mint was produced, as happens in a similar case with camphor, and the pitch was thrown down. The action of the red nitrous acid on it is violent; the acid is decomposed with great ebullition, and a portion of the pitch is converted into coal. In diluted nitric acid it dissolves, and produces an uniform brown fluid. On continuing to apply nitrous acid according to the process of Mr. Hatchett, solutions similar to those which he has described as having been obtained from the resins and bitumens, are produced.

I exposed a quantity of the pitch to a careful distillation through water. As might be expected from what I described before in the distillation of the tar, this process gave results nearly similar to the former. The oily matter differed in being of a brown colour and in having a greater specific gravity, and much less acid was produced; the residuum was charcoal. The whole process of distillation appears, therefore, to be a decomposition by which the pitchy substance is converted into oil, acetic acid, ammonia, and charcoal.

I proceeded next to examine the oil. It has a violently pungent taste and smell. It is scarcely heavier than water; so that it sinks in that fluid with difficulty, leaving generally some drops on the surface. It is perfectly soluble in alcohol, in ether, in caustic alkali, in olive oil, and in linseed oil. It will unite neither to naphtha, nor to the recent essential oils, but is soluble in the old ones. From these properties, it belongs to the class of the essential oils, but exhibits at the same time other qualities by which it is distinguished from the whole of them.

Having thus examined the most remarkable chemical properties of this substance, it will not be irrelevant to point out its differences from and its analogies with those substances which it most resembles, namely resin and the bitumens. Resin, as is well known, is eminently soluble in all the substances in which this is dissolved, and also in those with which this refuses to unite, even naphtha. But the general analogy between essential oil, turpentine, and resin, is so close to that of the three substances which I have described, that it will not perhaps be superfluous here to make some remarks on the nature of common resin and the substances connected with it, pitch, tar, turpentine, and essential oil, as their history will also illustrate that of the substance I am describing, and as it appears, like that of the bitumens, to have been somewhat mistaken.

If turpentine, as it flows from the fir in a liquid state, be exposed for a considerable time to the action of the atmosphere, it becomes brittle, and is converted into resin, in consequence, as it is supposed, of the absorption of oxygen. If the same turpentine be exposed to the action of the fire, a colourless volatile

oil

oil is separated, and resin remains in the retort. This however is not a mere case of the separation of a more volatile from a fixed substance; for a decomposition takes place, and acetic acid is generated. Nor can turpentine be again reproduced by mixing together the essential oil and the resin—it then forms a varnish. The essential oil is in fact a new compound, produced from the vegetable elements by the action of fire; and, although properly enough classed with those essential oils which are vegetable secretions, differs from them in some of its chemical properties. It is, for example, difficultly soluble in alcohol; but on exposure to air it becomes thick and yellow, and is then easy of solution in the same substance.

If the resin, which is the residuum of this distillation, be still further heated, it gives over a thick and high-coloured oil, gradually increasing in weight, till it equals, and at length exceeds, the specific gravity of water. The residuum becomes ultimately black, and very brittle, remaining soluble in ether and in lixivium of potash, but refusing to dissolve in alcohol.

Common tar differs from turpentine in containing a portion of the vegetable tar now under review, mixed with common turpentine and with the acetic acid which is formed in the distillation to which the wood is subjected for the purpose of obtaining it. Evaporation converts this into pitch, by decomposing it.

In this process, an essential oil, compounded of the oil of turpentine and the oil of wood, together with a portion of acetic acid, is separated, and the residuum, or common pitch, is a compound of resin and the wood pitch which I have been describing. To this admixture, and not to that of adventitious charcoal produced in combustion, is the black colour of common pitch owing.

The analogy between this wood pitch and the bitumens is equally striking, and the preceding history of these compounds will throw light on the several varieties of the bituminous substances.

Assuming the tar as the medium form, it is seen that when exposed to heat it gives over oil, and that pitch remains. Thus, petroleum yields naphtha and asphaltum; and thus too, asphaltum exhibits all the gradations which I have described in the pitch, its properties varying in a similar manner, according to its particular state. In the process of distillation, the principal difference will be found to consist in the relative quantities of acetic acid and ammonia, which they severally yield; the former chiefly characterizing the wood tar, and the latter the petroleum. From the same chemical cause which produces this effect arises also the difference in the nature of the inflammable gases which are produced from these different substances.

The sensible qualities of the bitumens (their taste and smell) are in all states utterly and entirely different from those of the vegetable tar. Petroleum is also much less soluble in alcohol, and further differs from the vegetable tar in being perfectly soluble in naphtha. In their solubilities in oil of turpentine they resemble each other, as well as in their habitudes with acetic acid and the alkaline lixivia, although the vegetable tar will be found the more readily soluble of the two. I need not repeat the circumstances in which the essential oil of wood differs from naphtha. It is a sufficiently characteristic one, that it forms no union with this latter.

It has been already shown that the difference between the pitch and asphaltum is considerable, when the former is in its first state, particularly with regard to its solubility in alcohol.

But if we compare the most brittle specimens of the pitch with common specimens of asphaltum, the differences, except as far as smell and taste are concerned, are not so apparent, and the reason of this will be obvious on considering their fundamental similarity of composition. The chief ingredients of both are carbon and hydrogen. By the application of heat, the proportions of these substances are altered in both cases, the hydrogen being abstracted in the greastest ratio, to form the new compound (the oil) in which hydrogen predominates. The ultimate result of both is charcoal. Asphaltum will be found to combine pretty nearly in the same way, with all the substances I have above enumerated as combining with the pitch. Its essential difference however consists in its solubility in naphtha, and by this test they are readily distinguished.

The chemical difference to which these different properties of substances so similar are owing, will be evident on considering some of the circumstances before related. The disproportion of acetic acid and carbonic oxide produced from the wood pitch, when compared with the produce of the bitumen, proves that it contains oxygen and azote in proportions different from those in which the same substances exist in the bitumens; and that in particular it contains a considerable quantity of the former. The result would not repay the toil required to investigate these proportions, which are probably also subject to considerable variation.

It is obvious that this substance is a new compound, formed by the action of fire on vegetable elementary matter; but all that we can determine of its nature is, that, in conformity to modern chemical nomenclature, it is formed of carbon, hydrogen, oxygen, and azote. The carbon and hydrogen constitute its basis, as they do that of the bitumens, and the large proportion of oxygen appears to give it the peculiar properties by which it is distinguished

distinguished from them. It seems evident, however, that no very great change is wanting to convert the one of these into the other.

The question so much agitated, of the conversion of vegetables into coal, would appear to receive some illustration from the history of the compound which I have been describing; and since (as I shall by and by show) it has actually been confounded with bitumen, and has been adduced as an instance of the artificial production of coal by the action of fire, I shall make no apology for pursuing this subject. Indeed the general chemical resemblance between the mineral bitumens and this vegetable bitumen, if it may be so called, is so striking, that we may at first sight be easily led to suppose that the same agent has produced both, and excuse the mistakes which seem to have occurred on this subject. But a cursory view of the several substances which have been classed under the head of bitumens, may enable us to form a clearer notion of the limited extent of this analogy, at the same time that it will perhaps assist us in correcting some errors which have crept into our arrangements of them.

It is necessary to separate from the bitumens three or four mineral substances, which differ completely both in chemical and ordinary characters, but which are approximated to each other by some general resemblance. These are, amber, mellilite, and the subterraneous resins of Cologne, Bovey, and Highgate. The two first are more nearly associated by the property they have of yielding a peculiar acid; and of the three last, it may perhaps be fairly doubted, whether they are more entitled to be ranked among the mineral substances strictly so called, than the other vegetable matters which are found in alluvial soils.

The nature and relations of naphtha, petroleum, maltha, and asphaltum, will, I trust, appear sufficiently clear from what I have above related; but I cannot forbear remarking on some false hypotheses which have been held respecting these substances, and their relation to other bodies. It is evident, from considering the products of their decomposition, that the basis of naphtha and of all the intermediate stages of bitumen, down to asphaltum, are carbon and hydrogen, modified by certain small proportions of oxygen and azote. It is in the relative proportions chiefly of these two ingredients that naphtha differs from petroleum, petroleum from maltha, and maltha from asphaltum. If we distil either of these more solid substances with a very gentle heat, we obtain naphtha, in which the proportion of the hydrogen to the carbon is increased to a maximum ratio. If the heat is greater, we obtain a substance of a darker colour, in which that ratio is less; and, for this reason, the distillation of asphaltum affords

a darker oil than that of petroleum, because its composition cannot be dissolved but in a higher temperature.

For the same reason also petroleum is easily rectified into naphtha. Asphaltum, in its ordinary state, contains the two ingredients in a ratio in which the carbon bears a large proportion to the hydrogen, and that ratio is reduced to the minimum, or becomes evanescent, when by the continuance of distillation charcoal alone remains behind. A large portion of the oxygen, and also of the azote, is disengaged during this process, but not the whole, since the darker compounds still give it over on repeating the process. The naphtha is probably entirely exempt from oxygen. With this view, we cannot accede to the notion that the absorption of oxygen is capable of converting naphtha or petroleum into asphaltum; or that the harder bitumens originate from the oxygenation of the more liquid. It is more consonant to the nature of these substances to suppose that the change consists in the alteration of the relative proportions of the hydrogen and carbon; but whether this is performed by the action of heat, or of other causes volatilizing the hydrogen, or by the contact of oxygen converting it into water, cannot now be determined by any facts that we are acquainted with. Experiments on the induration of the essential oils may throw some light on this question. It will here perhaps be remarked, that there is a difference in the substances as they are produced artificially by the distillation of coal, and as they are found in nature. Thus, for example, the artificial petroleum of coal differs from that of nature, in being much more soluble in alcohol. Yet this circumstance may arise from the insensible gradation of difference which I have above remarked in the similar compounds; and thus, in the series of gradation, specimens absolutely corresponding, whether artificial or natural, may exhibit the same chemical characters.

Thus, as I have shown that there is a sort of gradation from naphtha to asphaltum, through a series of undefinable petrolea, so this analogy may be extended to the next general variety of the bitumens, coal.

The several varieties of coal are supposed to consist of charcoal and asphaltum, or of charcoal and bitumen, combined in as many different proportions. Charcoal is undoubtedly found mixed with coal, but it does not appear correct to consider pit coal as either a mixture or combination of any bitumen with charcoal. The action of naphtha on its varieties, often none and always sparing, shows that bitumen does not exist in it in a mixed state. It will be more consonant to the analogies of the other bituminous substances, to consider coal in its several varieties as a bitumen.

a bitumen, varying in its composition, from the fattest specimens of Newcastle to the driest of Kilkenny, and owing its compactness, as well as the other modifications which it exhibits, to the peculiar circumstances under which it has been formed, the changes it may subsequently have undergone, or the substances with which it has accidentally been mixed. The power of yielding naphtha on distillation is rather to be resorted to as the distinction between the one end and the other of the series; and it would be surely equally correct to call coal a compound of charcoal and naphtha, as a compound of charcoal and asphaltum.

Its several varieties will moreover be found to vary from each other by containing greater or less proportions of carbon, compared with their other ingredients; just as in asphaltum the relative proportions of the hydrogen, azote, and oxygen, to the carbon, are found to differ from these which constitute petroleum or naphtha.

The last link of the chain of coal (chemically considered) is anthracite, which contains only carbon, if we reckon the earths mixed with it as adventitious matter. So the last result of the distillation of asphaltum is charcoal, and the intermediate steps through which asphaltum passes in its progress to charcoal, resemble precisely the corresponding changes which occur in the distillation of coal till coak is formed, and confirm by their chemical analogy the view here held forth of the chemical composition of coal, and the gradation to be traced in nature from fat coal to anthracite. If asphaltum be subjected to distillation it gives petroleum. By degrees its solubility in naphtha diminishes, in consequence of its carbonaceous ingredient becoming more disproportioned to its hydrogen. At a particular period of this distillation it will be found to resemble fat coal; by and by, it resembles blind coal, and gives no stain to naphtha; ultimately, pure charcoal remains. All these bituminous compounds may therefore properly be said to belong to one genus or family, of which the principal chemical ingredients are carbon and hydrogen; and it is chiefly to the difference in the relative proportions of those two substances that we are to look for the differences which characterize the several bitumens, from naphtha placed at one extreme, to anthracite placed at the other. The chasm in this series, from asphaltum to fat coal, is in fact rather apparent than real, being more properly a mechanical or accidental than a chemical or essential one I cannot here avoid taking notice of the very loose experiments of Mr. Kirwan on the analysis of coal, (which consisted in projecting portions of coal on melted nitre,) as his deductions are at war with this view of the subject, although not more so than with all chemical reasoning. They were founded on an assumption, that coal was carbon impreg-

 nated

nated sometimes with maltha and sometimes with asphaltum—a distinction quite unnecessary if the supposition were true. If we conceive coal to be compounded in this way, it would be more obvious to consider it as formed of carbon and petroleum, since by a regulated heat it can be separated into those two substances. The theory of the experiment is equally assumed, and the conclusions equally groundless, when it is inferred that of this compound (coal) the carbon alone possesses the power of decomposing the nitre, and that the proportions of these supposed ingredients may thus be determined. The varying temperature of the nitre would necessarily produce considerable variations and uncertainty in its action, and in the consequent accuracy of the results; but it is plain, that the effect of this contrivance was to separate by a sort of distillation the petroleum which fire elicits from coal, and that the method could neither be so accurate as that of ordinary distillation, from the greater irregularities to which it was subject and the difficulty of conducting it, and that it proves nothing with regard to the composition or nature of coal. In the examination of maltha, and asphaltum, the defect of this method is still more apparent. If heat and flame be applied to these bitumens, with access of air, they are either consumed without leaving any carbon, or that carbon which there is not oxygen enough present to burn, is deposited in a state of very minute division in proportion as it is volatilized, during the formation of the naphtha or petroleum, the more hydrogenous part of the compound. But if this part is separated without flame, either by a more moderate heat, or by excluding oxygen from it, the carbon is rendered apparent by its affinity of aggregation, which causes it in the end to assume comparatively refractory powers, and a more solid form.

Such are the views I would entertain of the bituminous genus, in which, as it is found in Nature, all traces of organization or resemblance to vegetable and animal inflammable matter have so thoroughly ceased, that we are entitled to give its several species a fair rank among minerals. But there is yet another division of inflammable and subterraneous substances connected with these, of which the claims may appear doubtful. Retaining as they do the traces of organization, and that sometimes in great perfection, it may be often questioned whether they do not more properly rank with the fossil remains than with the minerals properly so called. They are well distinguished by the name of Lignites. At one end of this series is placed jet, in which the traces of vegetable origin are nearly obliterated. Surturbrand and the several varieties of brown coal, including Cologne earth, connect it gradually with submerged wood and peat. The experiments I have already related prove that the substance

resembling

resembling bitumen, which is produced by the action of fire in the ordinary way on vegetables, differs from it essentially, and it has been seen that solubility in naphtha is the readiest criterion by which these substances can be distinguished. To assure myself of the accuracy of this test, I mixed the petroleum of coal with the black oil of wood in several proportions, and by the application of naphtha separated the one from the other. By this simple method, therefore, I expected to detect not only the progress of bituminization from simple turf to jet, but to assure myself whether, in the examinations hitherto made by others of these different substances, any mistake had arisen from confounding the vegetable bitumen with true bitumen when distillation was used to investigate their nature.

Vegetable turf in all its varieties, as well as brown coal, gave a considerable colour to lixivium of potash, but the same menstruum produced no effect on jet, or surturbrand. Nor had naphtha or alcohol any action except on the resinous lignite of Bovey, from which they extracted the resinous matter which that variety contains.

I therefore subjected these different substances to distillation, trusting that by the produce I should ascertain not only the fact but the progress of bituminization.

Submerged wood, from peat mosses in Cumberland, gave a brown oil, smelling of the wood tar, and refusing to dissolve in naphtha. In this case, therefore, no appearance of a change towards bitumen was exhibited. A compact pitchy-looking peat gave an oil which had a fetid smell, neither resembling that of wood tar, nor bitumen, and which was very slightly soluble in naphtha.

The Bovey brown (board) coal produced an oil of a peculiar smell, but most resembling that of wood tar, and much more soluble in naphtha than the preceding. Having a larger quantity of this, I separated the soluble part by naphtha, and in the remainder, or insoluble oil, the smell of wood tar was powerful, notwithstanding the strong odour of the naphtha. Here then the progress of bituminization had advanced another step. The resin of this wood, on which a particular name has lately been bestowed, I consider as an adventitious and accidental substance, and the natural produce of the tree, now probably unknown, which occupies these alluvial strata, as other lost productions of Nature are detected in other alluvial soils.

A specimen of black lignite from Sussex gave an oil which resembled the former in smell, and perhaps did not differ much from it in its solubility in naphtha; but I had not enough of the substance to institute an accurate comparison, neither in fact

 could

could it serve any purpose. A similar substance from Bovey gave similar results.

The oil which was distilled from jet was of a greater specific gravity than any of the preceding, and smelled strongly of petroleum. It seemed to be soluble in naphtha as readily as the specimen of petroleum with which I compared it. Indeed, had it not been that a greater quantity of acid was given over in this process than from any of the varieties of coal, I know not that any chemical distinction between the two would have existed. The mineralogical one is still considerable. The several specimens above enumerated, yielded each a large portion of acetic acid, marking as clearly as the peculiar sort of oil did, the remains of unchanged vegetable matter.

Examining therefore the alteration produced by water on common turf, or submerged wood, we have all the evidence of demonstration that its action is sufficient to convert them into substances capable of yielding bitumen on distillation.

That the same action having operated through a longer period has produced the change in the brown coal of Bovey, is rendered extremely probable by the geognostic relations of that coal. From this to the harder lignites, surturbrand and jet, the transition is so gradual, that there seems no reason to limit the power of water to produce the effect of bituminization in all these varieties; nor is there aught in this change so dissonant from other chemical actions, as to make us hesitate in adopting this cause. In the ordinary process of vegetable putrefaction and destruction, a variety of compound gases are formed by the reaction of their elements, and carbon alone, or rather carbon united to a portion of hydrogen, remains behind. Here the oxygen is completely dissipated, together with the azote, and the greater portion of the hydrogen. Analogous circumstances determine the putrefaction of animal matter; but in this case the play of affinities is so intricate, that a large portion of the carbon is volatilized in the gaseous form. By the constant affusion of water, however, this process may be so modified, that the greater part of the hydrogen and carbon will be retained, and enter, together with minute portions of other gases, into a new compound resembling fat, which has obtained the name of adipocire. The analogy is strong, and the gradual deoxydation of the wood in this process is visible in the different stages of bituminization.

Such, as far as observations have yet gone, is our knowledge of this process, and of the power of water in producing it. To repeat such an experiment in the laboratory seems impossible, since the necessary element of time must be wanting to complete it. But the action of fire being of shorter duration, and affording us

also

also readier means of imitating Nature in those operations in which she has wrought with the same agent, it is worth our while to consider, if by it we can produce from vegetables the bituminous matters under review. It is not necessary to say how intimately this question is connected with our speculations on the origin of coal, since Sir James Hall's experiments were expressly intended to illustrate this view of the subject. In this, it is related that "coal" was produced from "fir saw dust" by the usual method employed in these experiments, and that pieces of wood were changed "to a jet-black and inflammable substance, generally very porous," in some specimens of which "the vegetable fibres were still visible." There is no reason to doubt that the substance produced in these experiments was that black matter which I have described in the first part of this paper, which, however resembling bitumen in colour and inflammability, I have proved to be a different substance, and that the igneous theory of the origin of coal will receive no support from them, as far at least as relates to the conversion of vegetable matter into bitumen. I need take no notice of the modifications derived from a mixture of animal matter in these experiments, as it is not my desire to enter into a discussion of the general question, but to state such chemical facts as arose in the experiments I undertook. And since it is certain that vegetables alone are competent to the production of bitumen, and that the geological history of coal does not justify a supposition that animals have been concerned in its production, it is perhaps unnecessary to investigate that question further.

To satisfy myself whether any essential chemical difference would result from the experiments performed by simple heat, and those performed by heat under pressure, I repeated these trials, by heating wood in close gun barrels, introducing occasionally lime, clay, or other matters, to absorb the acid generated, and give the greater chance for the disoxygenation and bituminization of the wood. But the produce only differed from that of the experiments in open vessels, by the circumstance which is mentioned in Sir James Hall's paper, namely, the mixture of a porous charcoal, or a half destroyed vegetable structure. In all cases the bituminous-looking matter was vegetable tar, not bitumen.

Thus far then perhaps we are justified in concluding that the action of water, and not that of fire, has converted the vegetable matters into bitumen. It is another question to determine how that bituminous matter in its several forms of peat or lignite has been converted into coal, into a substance differing mechanically rather than chemically from it, if, without misleading, I may use the contrast of these terms.

[To be continued.]

XXXIX. *On*

XXXIX. *On the Electric Fluid.*

To Mr. Tilloch.

Sir,—In Read's "Summary View of the spontaneous Electricity of the Earth and Atmosphere" (1793) is the following passage:—"Some years since, I saw Mr. Volta obtain a very small quantity of electricity from the fumes of a mixture of water, oil of vitriol, and filings of iron." It appears to me that this mode of exciting the *electric fluid* bears great resemblance to a *galvanic* process. In this experiment there were two fluids and one metal used. The writer does not mention whether the ingredients were put into a metallic vessel. If they were, (which is not very probable,) there is a greater similarity to the usual *galvanic* processes, supposing the vessel made of a different metal from the filings used.

It is well known that electrical effects are produced by dropping a burning coal into a metallic vessel containing water. Does not this also resemble what is called Galvanism? I wish to induce persons to make both the above experiments, and accurately note down the particulars observable, and I think it probable that some interesting facts might result from the experiment.

If a number of vessels for the experiments were arranged like Volta's *couronne de tasses*, perhaps (for it is only conjecture) a considerable accumulation of the electric fluid might, as in Volta's experiment, be obtained.

I believe it has been wished by some electricians, that a single word should be adopted for the electric fluid. I know of no better than Fulgoric, the matter occasioning lightning; and although I am not very fond of new words, I shall venture to propose this. We might use *fulgoric* alone, or call it the *fulgoric* or *electric fluid* occasionally.

It appears to me that of late the two states of electricity, the *plus* and *minus*, have been considered as a state of *condensation* and *rarefaction*; which seems to be very proper; for do not many of the electrical phænomena bear a great resemblance to the phænomena observed in *pneumatic* experiments? By this I would be understood to mean, Do they not seem to be caused by a condensed elastic fluid rushing violently to mix with another portion of the fluid in a *comparatively* more rare state, and thence producing an *equilibrium* of density? If this opinion be just, may not (as has been before suggested in your Magazine) the conductor annexed to the cushion of a common *electrical machine* be compared to the receiver of a *rarefying air-pump*, and the conductor before the glass cylinder, to the receiver of a *condensing machine*, or, as it might be called, a *condensing air-pump*? The above is submitted to the consideration of your readers, hoping for their remarks.

March 20, 1815. A Friend to Physical Inquiries.

XL. *On Mr.* Bakewell's *Geological Section of the Northern Counties of England. By Mr.* J. H. Fryer.

To Mr. Tilloch.

Sir,—Upon looking over the last number of your very valuable Journal, I was much surprised to see a "Sketch of the Arrangement of the Rocks in the Northern Counties," with the name of R. Bakewell, Esq. attached to it, as it bears strong marks of having been copied from one made by me about two years ago. Now, though I am by no means anxious about being thought the author of so trifling a performance, yet having given copies of my sketch above twelve months ago to several mineralogical gentlemen (among others, to Mr. Winch, G. B. Greenough, Esq. and to the Rev. Mr. Turner, who exhibited it to his class in his course of lectures on mineralogy at Newcastle in February 1814), I feel myself called upon to assert my claim as the original author of the section, or to submit to the degrading supposition of having unfairly obtained a copy of Mr. B's, and passed it off as my own. Excepting the small part marked on the plate Mountains of grey wacke, &c. (my section showing the relative positions of the granite, syenite, &c. in that part) the whole of Mr. B's section is as exactly reduced from mine as could be done by a pentagraph. His section is also at variance with his letter;—at page 86, he disagrees with Dr. Thomson in dividing the coal formation; yet he exhibits that division upon his section: for the part marked "coal formation" on the plate is the Newcastle coal of Dr. Thomson, and the part marked "metalliferous limestone and other strata" is his independent coal formation. Though I disagree with Dr. T. in many points, yet I think the difference between the Newcastle coal and that in the metalliferous rocks is sufficiently characterized to allow them to be considered as separate formations, and had accordingly so divided them in my section. I acknowledge that in sections of a country, though made by different persons, there must be a great resemblance; but after what I have stated, I leave you to judge, how far *ours* are *necessarily* similar, and how far Mr. B. is borne out in his assertion, that his "is the first attempt that he is acquainted with to give a geological sketch of the succession of rocks across the island from the Irish channel to the German ocean."

I am, sir,
Your most obedient servant,

Lyzick Hall, March 21, 1815. J. H. Fryer.

XLI. *Pro-*

XLI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

March 2 and 9. The remainder of Sir Humphry Davy's paper On the Chemical Composition of the Paints and Colours used by the ancient Greeks and Romans, was read. It appears that ceruse was never used for white in any of the places examined by Sir Humphry, in the ruins of Pompeii, the baths of Titus, and other vestiges of ancient art, although the ancients were acquainted with the acetats. Their azure was prepared with nitre, flint, and copper filings. Sir Humphry states that 15 parts of soda, and 20 of flint, added to copper, produced their blues; cobalt was also used; minium, oxides of iron, and ochres, constituted their reds and browns; their whites were carbonates of lime; their glass sometimes contained manganese; and their blacks were all carbonaceous matter. The colour of the cuttle-fish was likewise occasionally used.

March 16. A long letter from Dr. Brewster to the President was read, detailing the particulars of Malus's discovery of the polarization of light reflected from the first and second surfaces of bodies, relating some of the author's first essays in this branch of physical science, and concluding with an *éloge* of the late M. Malus, and the merit or importance of his discoveries and researches. The Society then adjourned over two Thursdays till April 6.

SOCIETY OF ANTIQUARIES.

Mr. Lysons exhibited to the Society of Antiquaries, in the evening of the 16th of March, very accurate and finely coloured drawings of the mosaic pavement lately found in the remains of a Roman villa near Bignor in Sussex. The site of the building extended about 230 by 150 feet, and contained all the variety of apartments usual in Roman villas. In one apartment the mosaic consisted of squares laid over each other at right angles, and the intervening spaces filled with florets; in another, a pentagonal fountain in the centre, with a passage for carrying off the water; around the fountain were nymphs also in pentagonal compartments, many of them very well executed. The whole were inclosed in a circle more correct than usual with Roman artists. Others consisted of mosaics with tolerably fine heads of Venus in the centre, represented no doubt as beautiful as the artists of the age of Agricola (the period at which Mr. L. supposes that this villa had been erected) could execute.

WER-

WERNERIAN NATURAL HISTORY SOCIETY OF EDINBURGH.

March 11. A paper by Mr. Scoresby, of Whitby, was read, on the subject of polar ice, and the difficulties encountered by the whalers in their annual visits to those northern regions.—Mr. Scoresby's frequent visits to that part of the globe have induced him to turn his attention to the possibility of reaching the pole by means of an excursion over the ice; and a considerable portion of his paper was devoted to this very interesting subject. It appears, from his accounts, that the whalers have sometimes penetrated so high as 81½ degrees, or within about 600 miles of the pole, which space he considers might be travelled in sledges, by means of rein-deer or dogs, in about a fortnight; and that for the return, allowing for casualties, and time for observations, the whole might be accomplished in six weeks. From his knowledge of the climate, he thinks the temperature of the weather will present no very great obstacle, having observed that after a long continuance of north wind the thermometer never was very considerably depressed. The difficulties he contemplates are, open water, rough or mountainous ice or similar land, soft snow, and dense mists.

As it is supposed that the first will not be very extensive, he proposes to have his trainaux formed in the shape of canoes, so that they might pass over any thing of the kind, while the dogs might be made to swim; and for other obstacles, he must in a great measure trust to chance, supplying himself with whatever may be necessary to overcome them, should they occur.

In shaping his course to the pole, he must soon lose the assistance of the magnetic needle, and must trust to solar observations, which, together with a good chronometer, would answer every purpose, could he be sure of not meeting with mists. He likewise proposes to assist himself in misty weather by making his cavalcade move at some distance apart in a straight line, which once properly directed may, with attention, be got to proceed with tolerable accuracy. From his experience of the atmosphere of high northern latitudes, however, he does not anticipate such weather, except with southerly winds, which are not of frequent occurrence or long continuance.

It is very satisfactory to observe a subject of this kind treated with so much ardour, and at the same time with so much science, as Mr. Scoresby has done. It is an object which has long been very much desired by men of science, to ascertain the nature and form of the globe at the pole, and the manner in which the needle is acted upon in proceeding towards it. This, however, is not an object that can be taken up by an individual; and if, after explaining his views to men of science, his plan may be pronounced

pronounced practicable, of which there seems to be no doubt, we hope that Government will second his intentions, and provide for the expenses of such an arduous undertaking.

KIRWANIAN SOCIETY OF DUBLIN.

Feb. 22, 1815. The reading of an Essay was commenced, "On the Origin, Progress, and present State of Galvanism, with Observations on the Inadequacy of the Hypotheses proposed to explain its Phænomena, and Reflections on the Influence of Galvanic Discoveries on the Doctrines of Chemistry;" by M. Donovan, Esq. Secretary*.

The sketch of the history of Galvanism, with which the Essay commenced, was divided into three periods: 1st, containing the discovery of muscular contraction effected by simple Galvanic contact, and of combined Galvanic arrangements; 2d, containing the gradual development of the physical and chemical powers of combined Galvanic arrangements; and, 3d, containing generalizations of the chemical effects of Galvanism, and the discoveries that have resulted from the application of these generalizations to research.

Period 1. The first distinct Galvanic phænomenon was the taste excited on the tongue by two metals in contact, neither of which could separately produce any effect; a fact noticed by Sultzer in 1767, and little attended to until in 1791 it was found by Galvani that metallic contact excited a surprising artificial motion in the muscles of dead animals. Shortly after, the experiments of Sultzer and Galvani were attributed by Fabroni to the operation of the same agent. Galvani considered the agent to be a kind of electricity inherent in animals: Fabroni maintained the electricity to be concomitant, and the taste and contractions to be produced by chemical action of the metals. The discoveries and opinions of those who embraced the doctrine of either of the above philosophers, with or without modifications, were then stated. Humboldt denied that the agent was electricity, as it was not conducted by various good conductors of that fluid. Pfaff considered the agent to be *sui generis*, and associated or identical with the principle of life. Fontana denied that muscular motion is in any degree connected with electricity. Valli adopted the opinion of Galvani, with a peculiar modification: he allowed that electricity was the agent, but believed that it was identical with the nervous fluid. From this Monro dissented: he thought that the unknown agent merely acted as an artificial stimulus to the *natural nervous fluid*. Volta differed

* This Essay in a somewhat different form was honoured with the prize by the Royal Irish Academy.

with

with all the preceding: he admitted that the agent in contractions is electricity, not inherent in the animal, but generated by the contact of two metals, which he found it necessary in most cases to employ. Vassali-Eandi preferred the opinion of Galvani, but added this modification, that the metal employed is not a generator but a conductor of the electricity, which is evolved during chemical action continually taking place in animals, and not inherent in them as a principle. Fowler denied that the circumstances for producing electricity are present, and the phænomena are even reconcileable to the laws of fluid: he supposes that the influence, whatever may be its nature, is not derived from the metals alone, but partly from the animal. Experiments of Smuch, Marsigli, Grapengiesser, Giulio, Rossi, and Aldini, were then noticed. Wells allowed that electricity was the agent, but denied that it was produced by the contact of the metals, or by any inherent quality of the muscles and nerves. At length Volta proved to a demonstration, that by the contact and separation of different metals electricity is really evolved; a fact which in some time after led him to the grand discovery of the pile,—a column erected to the memory of its inventor, and inscribed with an epoch in the annals of knowledge.

March 8.—*Period* II. The second period was commenced with the discovery of the Galvanic decomposition of water by Nicholson and Carlisle. Mr. Cruickshank effected many decompositions, as of metallic and earthy salts: he drew up some general conclusions; and gave a new form to the pile, which combined convenience with increase of power. Mr. Henry decomposed sulphuric and nitric acids, and submitted other substances to examination. Colonel Haldane made many experiments upon the metals most proper to form Galvanic arrangements, but could not believe that the agent is electricity. Mr. Davy repeated the experiments on the decomposition of water, and ascertained some very singular circumstances of it: he found that a fluid interposed in the pile, which can act chemically on the plates, greatly increases its power. Ritter, struck with the separate evolution of the gases in the decomposition of water, was led to deny the constitution of that fluid, and to substitute some singular opinions for the received theory. Robertson considered the Galvanic influence peculiar, and of an acid nature. Brugnatelli also supposed that it is an acid, but identical with electricity. Fourcroy, Vauquelin, Thenard, and Lehot denied the identity of the Galvanic and electric fluid: they heated wires red hot by means of large plates. Tromsdorf and Simon effected the combustion of metals. Helwige, Twast, Bourguet, Erhman, and Grapengiesser accomplished the combustion of inflammables.

Attempts

Attempts were made by Dr. Wollaston to reconcile the contending hypotheses of Fabroni and Volta: with the latter, he supposes electricity to be the agent; but, with the former, he considers chemical action to be the exciter, and extends oxidation to the explanation of every case of electrical excitement. Davy showed that oxidation was not the sole exciter, for sulphuration produced Galvanic appearances: this had been before shown by Volta. The identity of Galvanism and electricity was attempted to be proved by Van Marum and Pfaff, by comparing the effects of the pile with the common electric machine: they concluded that oxidation was unnecessary. Biot, Cuvier, and Desormes proved that oxygen is absorbed by the pile in action. Bostock admitted that oxygen is necessary to the evolution of electricity in the pile, and conceived that hydrogen is essential to its conveyance, a combination taking place between both. Cuthbertson considered the production of electricity as depending on the metals in contact, and on the chemical action. Aldini and Lagrave showed that neither metallic contact nor chemical action on metals was necessary, by producing Galvanic effects with different kinds of animal matter only; and Jordan and Ritter conceived that Galvanic arrangements could be effected with magnets. This period of the history concluded with the labours of Dr. Wilkinson.

March 22.—*Period* III. The third period of the history of Galvanism commenced with the well known generalizations of Hisinger and Berzelius; their experiments on the invisible transfer of elements to a distance; and the explanation given by Grotthus of the invisible transfer of the elements of water. The announced decomposition of muriatic acid by Peele, Pacchioni and others, was then detailed, and the discovery of the source of mistake by the Galvanic Society, Pfaff, Biot, Thenard, and Davy. Mr. Sylvester's well supported opinion, that the precipitation of one metal by another is a Galvanic phænomenon, and his explanation of it, which was afterwards enlarged on by Grotthus, was then stated. Mr. Wilson's experiments with metallic filings were conceived very much to weaken the grounds of the Voltaic hypothesis, which supposed the electricity to be evolved by contact: Mr. Wilson showed that separation alone produces electricity: on the other hand, the Voltaic hypothesis acquired new probability from the experiments of Mr. Erhman, in which it was proved that Galvanism and electricity are conducted by the same bodies, and that the supposed anomaly arose from not being acquainted with a new class of conductors, which are such to one pole only of the pile. The grand discoveries of Davy relating to the alkaline metals, and of Berzelius, Pontin, and Davy with regard to the earthy metals, were detailed; also the production

duction of potassium in a different manner by Gay Lussac and Thenard; and their researches with Davy's on the bases of boracic and fluoric acids, and on the simple combustibles. The accounts were next detailed of Mr. Children's battery of large plates which possessed intense heating power with no electric effects; and also Mr. De Luc's and Mr. Singer's column, which possessed high electrical intensity with no other power. Lastly, the objections offered to Mr. Davy's hypotheses concerning oxymuriatic acid, and electro-chemical affinity, by Mr. Murray, Dr. Maycock, and the author, were stated, along with Mr. Murray's opinion on the nature of the alkaline metalloids.

[To be continued in our next.]

ROYAL INSTITUTION.

*Mr. Brande's Lectures**.

Mr. Brande, in his fifth lecture on the history of chemical philosophy, entered upon that period of his subject in which the nature of the atmosphere began to attract the notice of chemists. As in other branches of experimental science, so here the advances of the human mind had been very gradual. Mayow in 1674, said the professor, was upon the very brink of that torrent of discovery, which in 1774 carried Dr. Priestley into the fastnesses of pneumatic chemistry. Hales, by showing the mode of disengaging and collecting gaseous fluids, had removed all serious obstacles from this path of research; he was followed by Boerhaave and afterwards by Black, who, having discovered fixed air, turned into another road of investigation. Although these philosophers, therefore, had opened the mine, they neglected to explore it—its treasures were reserved for more diligent and dexterous experimentalists. Mr. Brande then cited the opinions of Libavius, Rey, Mayow, and Hooke, respecting the nature of the atmosphere, and added a brief account of what had been done in this insulated branch of inquiry, previous to the year 1772, when Dr. Rutherford, of Edinburgh, demonstrated, that after the separation of fixed air from that contaminated by respiration and combustion, the remainder was still noxious: to this new variety of air the term azote had been applied; it was also called nitrogen, it having been afterwards recognised as a component part of the nitric acid.

The discovery of azote was doubtless an important step in pneumatic chemistry; Mr. Brande passed on to that marked epoch of chemical history, which was adorned by the names of Priestley, Scheele, Cavendish, and Lavoisier.

"These discoveries," said the professor, "led to new views of

* We regret that our limits do not admit of our giving the whole of this ingenious and popular gentleman's lectures *seriatim*. We have therefore confined ourselves to such of them as appeared of the most general interest.

of the physiology of vegetables, and strikingly displayed some of those masterly and beneficent adjustments of nature by which the different members of the creation are made to minister to each other's wants, and thus preserve that eternal harmony which marks the natural world."

Mr. Brande said it was a curious fact that some plants effected the same changes upon the atmosphere as animals, only in a much less degree: he alluded to some experiments of his own, in which similar portions of contaminated air were exposed to the operation of the vine, mint, the pea, and water cresses, all in a healthy state of vegetation: by the vine and mint it was purified, unaltered by the pea, and rendered less pure by the cresses. Mr. Brande hoped upon these grounds to be able to explain some discordant results of later experimentalists.

The remainder of the lecture was occupied with the details of Dr. Priestley's greatest discovery, that of dephlogisticated air, or, as it has since been styled, oxygen gas, on the first of August, 1774.

In his sixth lecture Mr. Brande resumed the history of Dr. Priestley's researches. Foiled in his attempts to collect and preserve certain gases over water, Dr. Priestley devised the expedient of confining them over quicksilver. It was thus that he obtained muriatic acid, sulphurous acid, and the volatile alkali, in their pure elastic form: he then imagined, that by mixing the acid and alkaline air he should obtain a neutral aëriform fluid; but to his surprise a solid salt resulted, and the gases were wholly condensed. Having illustrated these and some other inquiries of minor importance, by simple but satisfactory experiments, the professor took a general view of Dr. Priestley's scientific character and discoveries.

Mr. Brande next proceeded to illustrate the discoveries of Scheele; he dwelt principally upon that of dephlogisticated muriatic acid, a subject that has given rise to a very singular controversy in the chemical world. Scheele regarded it as an elementary substance, as the *base* of muriatic acid: it was next examined by Berthollet, who imagined that it contained oxygen, and was composed of that principle *united to* muriatic acid. Sir H. Davy had found reason to doubt the accuracy of these conclusions, which for twenty years had prevailed in the chemical world, and by a series of satisfactory inquiries had re-established the correctness of Scheele's original views. Mr. Brande said, that any extended discussion upon this subject would be misplaced in his present lecture; but it had been alluded to in a manner neither complaisant nor philosophical, in a recent French publication—and whenever the claims of his eminent predecessor were unwarrantably attacked and infringed, it became his imperious duty to step forward in their defence.

Some

Some beautiful experiments illustrating the properties of dephlogisticated muriatic acid, or *chlorine*, (as it had been called by Sir H. Davy, in consequence of its peculiar colour, and to avoid all hypothetical reference to its composition), were exhibited, and its application to the purposes of bleaching concisely illustrated. Mr. Brande feared that its application to whitening rags for paper-making, had been productive of some mischief; and that the spots and blemishes of the paper common in many of the magnificent productions of our press, might be referred to the effects of this substance.

On the other discoveries of Scheele, noticed by Mr. Brande, our limits prevent us from dwelling: he particularly alluded to that of the fluoric acid; and relieved the narrative and illustration of Scheele's discoveries with some admirable remarks upon his character and genius as a philosopher.

FRENCH INSTITUTE.

Monday, January 9, 1815, the anniversary meeting of the Class of Mathematical and Physical Sciences of the French Institute took place. The principal object of the meeting was the distribution of prizes. M. Lalande's annual astronomical prize for the most interesting observation or the most useful memoir on the science of astronomy, was adjudged to M. Piazzi, astronomer royal, of Palermo, for his ample catalogue of nearly 7500 stars published in 1814.

The two prize subjects for 1817 are: first, To determine the action of the thermometer filled with quicksilver, at least from zero to 200° centigrade; the law of refrigeration in vacuo, the laws of refrigeration in the air, hydrogen, and carbonic acid gas, at different temperatures and in different states of rarefaction: secondly, To determine the chemical changes which take place in fruits during and after fructification.

The Class will also adjudge in the public sitting for 1816, a prize for the best work or memoir, in print or manuscript, on the application of mathematical analysis to a question in physics, or to the best experiments in general physics, with which it shall be made acquainted previous to October 1, 1815.

M. Delambre then read a biographical notice of Abbé Bossut and M. Cuvier of Count Rumford; when the Class adjourned.

At the meeting of the Class of Physical and Mathematical Sciences on the 23d of January 1815, a report was read on a memoir by M. Le Pere, inspector of roads and bridges and member of the Institute of Egypt, respecting the ancient communications between the Indian ocean and the Mediterranean by the Red sea and the isthmus of Suez. Speaking of the result

of the great operation of levelling the two seas by the isthmus of Suez, M. Le Pere, or rather the reporter of his memoir, observes: "This levelling resolves the celebrated question agitated since the days of the ancients respecting the elevation of the Red sea above the Mediterranean sea and the soil of Lower Egypt: we thereby discover that the low-water mark of the Mediterranean is lower by eight metres and 121 millimetres than the low-water mark; and nine metres and 907 millimetres lower than the high-water mark of the Red sea. We also find that the total slope of the hill from Cairo to Rosetta, in a distance of 252,000 metres, varies by about eight metres from the lowest to the highest level of the waters: the mean declivity when the river is at its lowest ebb is $\frac{5285}{252000} = 0,0002$, and at its fullest state in September 1798 this declivity became $\frac{12865}{252.000} = 0,00051$.

"The difference between the high and the low-water mark at Suez is one metre 786 millimetres: the Nile in its most swoln state at Cairo is superior at first by eight metres 960 millimetres, and in the second stage by four metres 740 millimetres: at its lowest state at the same place it is inferior to the low-water mark at Suez by two metres 836 millimetres.

"The point in the vast basin of the bitter lakes is remarkable for its being nearly eight metres below the low-water mark of the Red sea: other points of land, and even places which are inhabited, are below the level of both seas; and an immense extent of ground, very little elevated above the Mediterranean, is far below the Red sea; so that the waters of the latter might cover the surface of the Delta, and the well founded fears of this submersion may have caused great alarm at distant epochs, when the Delta was still lower than it is at present."

M. Le Pere and the Council of the Institute, to whom his memoir was referred, conclude by asserting, that it is quite practicable to re-open the communication by means of canals between the Red sea and the Mediterranean.

Abstract of the Labours of the Class of Mathematical and Physical Sciences during the Year 1814. *By M.* Cuvier, *perpetual Secretary.*

"The memorable events of which the French capital has been the theatre, far from interrupting our scientific researches, have furnished new proofs of the respect which the sciences inspire, and of the great influence which they have acquired over men of all ranks and professions. Innumerable armies from the extremities of Europe have visited our monuments, have examined our collections, without their sustaining the slightest damage. Friends of science, enrolled in this grand crusade, undertaken

in

in a great measure for the restoration of the liberty of writing and thinking, had scarcely laid down their arms, when they flocked to make themselves acquainted with our labours, to take a share in them, and to instruct us as to what was done among themselves."

The reporter commences with chemistry, and notices the discovery of *iode*, recapitulating its properties.

"When Lavoisier and Berthollet formed the new system of chemistry on their beautiful experiments, which demonstrated their luminous theories of combustion, the formation and decomposition of water, and of the formation of the acids by the combination of oxygen with different bases; when this truth was completely established by evidence, so far as the principal acids were concerned, it was inferred from analogy that it might be extended to all. Nevertheless, some scruples arose on this subject, as soon as it was proved by M. Berthollet that sulphuretted hydrogen had the properties of the acids, although oxygen was no part of its composition. It was acknowledged that oxygen was not the only principle capable of operating acidification.

"The muriatic acid, that acid which combined with soda forms common salt, had not been decomposed: and the oxygenated muriatic acid, a substance so precious from the property which it possesses of disinfecting the air by neutralizing putrid miasmata, and from its use in bleaching,—this substance, which had been long regarded as a combination of the muriatic acid with a superabundance of oxygen, may be regarded as a simple substance, from a variety of concurrent circumstances, as a burning (*comburan*) or acidifying principle as well as oxygen: and most chemists who have adopted this opinion, those who are considered as the best authorities, have called it *chlore*, from the yellowish colour which they found it to possess. The muriatic acid, according to them, is a combination of *chlore* with hydrogen, and they call it *hydrochloric* acid.

"Iode also forms acids by being combined with hydrogen and other substances: we must therefore place it with chlore and sulphur among the number of acidifying principles.

"A fourth principle is sought to be discovered in the fluoric acid, and hitherto not decomposed. This acid is remarkable for its property of corroding glass, so that it can only be kept in flasks internally coated with wax. This property has been taken advantage of for making drawings upon glass by means of the fluoric acid reduced into vapour. We presume that it results from the composition of hydrogen, with a simple body of a peculiar nature, and which we shall call *fluor*. It was M. Ampere, a most eminent geometrician, who first conceived this idea, and it is for professional chemists to demonstrate it.

 "Will

"Will all these discoveries revolutionize chemistry once more? The new chemistry, wholly founded on experience, cannot change. The old chemistry, completely systematic, had various foundations upon which it could not stand. The new chemistry advances and enriches us as it proceeds; but it changes nothing notwithstanding."

M. Theodore de Saussure has demonstrated, by the decomposition of alcohol or spirit of wine and ether, that these two substances are composed in the same way as carbon and hydrogen, but combined with different portions of water reduced to its elements, so that in alcohol these elements form one-third; whereas in ether they form only one-fifth! Thus, when by distillation of alcohol with sulphuric acid we obtain ether, it is because the acid has taken from the alcohol a portion of its water. This same acid, if we employ it in a greater quantity, will take up the whole of the water, and will reduce the alcohol to the state of olefiant gas, a kind of carbonated hydrogen gas highly charged with carbon, and to which this name has been given, because in burning it deposits oil.

M. Mongez, in a memoir on the bronze of the ancients, has proved from the experiments of M. Darcet, that it was not by immersion in cold water that the bronze hardens, as is the case with steel, but that it acquired its hardness when after being made red hot it was allowed to cool slowly in the air. M. Darcet has made cymbals in this way of an excellent tone, and which it was pretended could only be made in Turkey, and by one workman of Constantinople.

The falls of atmospheric stones, now that their reality has been ascertained, have been so often observed, that the long incredulity which people entertained on the subject, will speedily be the only topic for surprise connected with them. There was a very remarkable phænomenon of this description in the department of the Lot and Garonne, on the 5th of September, attended as usual by a serene sky, a great explosion, and a whitish cloud. The number of stones was considerable, and they were dispersed over a radius of a league.

M. Cuvier has examined at Haerlem a petrified skeleton, extracted more than a hundred years ago from the quarries of Œningen, near the lake of Constance, which Scheuchzer, a naturalist of Zurich, had taken for that of a man, and which he had engraved as "the man who was a witness for the Deluge." M. Cuvier has ascertained that this skeleton belonged to an unknown and gigantic species of Salamander, as he had already announced, on a simple view of the engraving, in his great work on fossil animals. From not meeting with the fossil remains of human beings, M. Cuvier thinks that man is the newest inhabitant of the globe.

M. De

M. de Humboldt has given the truly wonderful history of the volcano of Jorullo, which opened in 1759, at Mexico, on a well cultivated plain, through which two rivers ran, and where no subterraneous noise had been heard within the memory of man. The catastrophe was announced some months before by reverberations which lasted fifteen or twenty days. Flames and showers of ashes determined the inhabitants to fly: the surface of the ground rose and fell like waves, and a multitude of small cones issued, from six to nine feet high, and which still exist. At length there arose a series of six hillocks, the principal of which has still an inflamed crater, and is not less than 1600 feet high. Every morning the smoke rises from small cones and crevices; and the water of both rivers is hot, and impregnated with sulphuretted hydrogen. This volcano, like several others of the new world, is upwards of 40 leagues from the sea; whereas on the old continent we know of none further removed than twelve leagues.

Botanists are not yet agreed as to the fructification of the mosses. Linnæus, who established his system according to the several organs, unable to discover them in the mosses, mushrooms, lichens, &c. ranged all these plants in a single class, which he called *cryptogamia*, or secret marriages. The mosses flourish in winter; on a very delicate pedicle rises a small urn, at first covered with a conical cap closed by an operculum; the cap falls, the operculum is detached, and the urn is found full of a green powder which Hedwig considers as the seed. M. de Beauvoir has made some new observations, which induce him to believe that the powder or dust in the urns is a true pollen or fecundating powder, like that of the stamina of the greater part of plants, and according to him, the seed is contained in a small column which forms the axis of the urn. Hence the mosses, like the greater number of other vegetables, are hermaphrodites.

M. Lamouroux, of Caen, has published several memoirs on marine plants, considered with respect to their use for the nourishment of men and animals, in political and domestic œconomy, in the arts and comforts of life. It is surprising how many useful or agreeable articles different nations have derived from vegetables so little distinguished; some are directly eaten, or converted into a savoury and nourishing jelly; others furnish an essential support to animals in the frozen regions of the North, and all of them yield soda or manure. A few yield sugar, and others colouring matter for the dye-house; several of them are made into mats, vessels for drinking, and even musical instruments: that called moss of Corsica is a valuable remedy.

M. Desvaux has distinguished forty-four varieties in the common species of the banyan tree; he has also collected 172 va-

 rieties

rieties of the fig-tree cultivated on the shores of the Mediterranean, although he has not yet visited Languedoc: he has designed, coloured and described them minutely.

M. Tollard, in his history of useful vegetables introduced within the last ten years into French agriculture, states that the *Dalhia*, a new plant, the flower of which is a beautiful ornament, is more important for its roots, which are larger and almost as good to eat as the roots of the *topinambour*.

[To be continued.]

XLII. *Intelligence and Miscellaneous Articles.*

MIGRATION OF BIRDS.

Manchester, March 4, 1815.

Sir,—The last volume of the Memoirs of the Philosophical Society of Manchester contains a valuable Essay on the Migration of Birds, by Mr. Gough, of Kendal. Since the appearance of this paper I have made some inquiries respecting migrating birds, and find it very difficult to meet with any thing satisfactory on the subject, from any other part of Europe. It seems that few British travellers, or even foreign naturalists, have paid that attention to these transitory visitors which the subject deserves; for I am at a loss to know where a series of accurate observations respecting them can be met with, besides those stated by Mr. Gough. It is very desirable that their regular appearance and disappearance in the *different latitudes of Europe* should be ascertained, as it would considerably illustrate the history of this class of the feathered tribe. It would, I believe, be interesting to many of your readers, if you could procure, through the medium of foreign publications, or from your extensive correspondence abroad, an account of these birds from any other part of the globe. If such account was somewhat similar to Mr. Gough's, it would either throw further light on his theory, or else produce facts that might be combined with such circumstances as may tend to render this dark and controverted subject more clear. By inserting the above in your valuable Journal, it may draw the attention of some of your numerous correspondents to this amusing subject; and will oblige,

Sir, your obedient servant,

To Mr. Tilloch. S. K.

MINERALOGY.

Newcastle-upon-Tyne, March 8, 1815.

Sir,—In your last number I have noticed two questions put to me, respecting facts stated in a paper I transmitted last year to

to the Geological Society, to which I shall answer as briefly as possible. The muscle shells found in the shale of this part of the kingdom are mineralized by clay iron stone, and exactly resemble the common fresh-water muscle in shape, but are less in size. I beg leave further to add, that no organic remains which I could suspect to have once belonged to marine animals, were ever observed by me in the Newcastle coal formation, though the magnesian limestone by which it is covered, and the lead mine sills lying below it, abound with them. With regard to the grindstone sill on Alston Moor, it consists of a sandstone not unlike the stratum from which grindstones are quarried in this vicinity, but the former must be situated far below the latter. As Mr. W. Forster's section is referred to in the letter of my anonymous correspondent, permit me to say, that in the year 1800 a similar engraving was published at Carlisle (and *signed William Millot, miner*), of all the lead mine measures; and previous to that period the late Mr. G. Johnson had an engraving made of the strata sunk through at the coal mine situated at Byker St. Anthony. These two, together with the section of Sheriff-hill Colliery, printed in Hutchinson's History of Durham, furnish out the section given to the public by Mr. Forster in 1812. MSS. of all these documents have been in the library of the Literary and Philosophical Society of this place for twenty years past. For the directions and description of the lead veins, which are certainly highly valuable in a geological point of view, the world are indebted to Mr. Forster.

Your obedient servant,

To the Editor of NAT. JOHN WINCH.
The Phil. Mag. and Journal.

MATHEMATICAL QUESTION.

Plymouth, March 11, 1815.

SIR,—I HAVE been long baffled in my attempts to integrate the following differential, viz. $\frac{\dot{\delta}}{\delta^{\frac{3}{2}} - (\delta - \alpha)^{\frac{3}{2}}}$, which occurs in a very interesting department of Hydrodynamics, at art. ccccliv. page 418, of Dr. Gregory's excellent Treatise of Mechanics. It is possible that some of your learned correspondents may be able to accomplish the integration, and you will therefore much oblige me by inserting this in your next number.

Yours, &c.

To the Editor of GEORGE HARVEY.
The Phil. Mag. and Journal.

*** Our limits oblige us to entreat any of our correspondents who may answer Mr. Harvey, to be as brief as possible.

OPTICS.

OPTICS.

SIR,—THE following question has been proposed to many philosophical persons, without obtaining any satisfactory answer, notwithstanding the importance of the inquiry in regard to the construction of astronomical instruments; I therefore beg leave to ask your readers, "What is the smallest angular space that is perceptible to a sound unassisted eye, or to an eye assisted by a telescope of a given power, aperture, &c.?"—In Dr. Gregory's Œconomy of Nature, vol. i. 238, I read, "Opticians *say* that the eye is not capable of perceiving any object which subtends an angle of less than half a minute of a degree: the image on the retina is in this case less than $\frac{1}{7200}$dth part of an inch, and the object itself, at six inches distance, less than the $\frac{1}{1200}$dth part of an inch broad. All smaller objects are invisible." Lalande in his Astronomy says, Eight minutes, 8′, are imperceptible on a figure of a foot radius: "8′ sont insensibles même sur une figure d'un pied de rayon, telle que j'ai coutume de l'employer*," in projecting the occultations of stars. The foot English = 11 inches 3·1154 lines French. This is all that I have found in answer to the above question, and this is not sufficient. I desire to know also on what experiments or observations such assertions are founded.

To Mr. Tilloch. A. M.

HYDRAULICS.

General Andreossy, the late French ambassador at Constantinople, has communicated to the First Class of the Institute various researches, accompanied by charts and drawings, respecting the conduits which supply Constantinople with water. They contain descriptions of the ancient and modern structures on principles not hitherto known, and the application of which promises to be highly advantageous in hydraulics, both with respect to the simplicity of the contrivances and the œconomizing of labour.

EFFECTS OF SEVERE COLD.

M. Desgenettes, the celebrated French physician, who accompanied the unfortunate army which penetrated into Russia, thus describes some of the phænomena which occurred among the troops who were exposed to the intense cold which was fatal to so many thousands, during the retreat from Moscow. The effects alluded to were perfectly new to M. Desgenettes, and will doubtless be equally so to our medical readers: "I have heard

* The radius of an arc being unity, the length of a second = 0·00000 48481 36811 09535 99359.

men,"

men," says this acute observer, "who were marching with every appearance of muscular energy and with the most decided and soldier-like pace, suddenly complain that a thick veil was covering their eyes: those organs, at first, for an instant haggard, soon became immoveable: all the muscular apparatus of the neck, and more particularly the sterno-cleido-masto-idean muscles became rigid, and gradually riveted the head on the right or left shoulder: this rigidity next extended to the trunk; the lower extremities tottered, and the unhappy victim fell upon the snow, exhibiting, to complete the frightful picture, all the symptoms of catalepsy or epilepsy."

LIGHT.

The Moniteur, of January 15, contained the following short article, by M. Biot, "On the nature of the forces which produce double refraction."

"When a ray of light penetrates a crystal, the primitive form of which is neither the regular octohedron nor the cube, we observe in general that it is divided into two fasciculi unequally refracted. The one which we call the ordinary fasciculus follows the law of refraction discovered by Des Cartes, and which is common to all crystallized and non-crystallized bodies: the other follows a different and more complex law: it is called the extraordinary fasciculus.

"Huyghens has determined this last law by observation in the rhomboidal carbonate of lime, vulgarly called Iceland spar, and he has described it by a construction equally ingenious and precise. By combining this fact with the general principles of mechanics, as Newton has combined the laws of Kepler with the theory of central forces; M. Laplace deduced from it the general expression of the velocity of the luminous particles which compose the extraordinary ray. This expression indicates that they are separated from the others by a force which has emanated from the axis of the crystal, and which in the Iceland spar is found to be repulsive.

"It was generally supposed that this was the case in all the other crystals endowed with double refraction. But new experiments have proved to me that in a great number the extraordinary ray is attracted towards the axis instead of being repelled. So that with respect to this property the crystals onght to be divided into two classes; the one I call *double attractive refraction*, and the other *double repulsive refraction*. Iceland spar forms part of the latter: rock crystal is comprehended in the former. Finally, it appeared to me, that the force, whether attractive or repulsive, always emanates from the axis of the crystal,

stal, and always follows the same laws: the formulæ of M. Laplace therefore apply to them always.

"Some previous inquiries had already led me to recognise a singular opposition in the nature of the impression which various crystals give to light in polarizing it. I had expressed this opposition by the terms *quartzous polarization* and *berillated polarization*, from the substances which had first exhibited this opposition. I have now ascertained that all the crystals endowed with the quartzous polarization are attractive, and all those which exercise the berillated polarization are repulsive. Iceland spar belongs to the latter class.

"These results show that there exists in the action of crystals upon light, the same opposition of forces which has been already recognised in several other natural actions, such as the two kinds of magnetism, and the two kinds of electricity. To this also the other observations lead, which I have already published on the oscillations and rotations of luminous particles."

CHOAK AND FIRE DAMP IN MINES.

Dr. Watt, of Glasgow, informs us that a Mr. Davidson, who has been blind from childhood, is now delivering lectures in Scotland, on natural philosophy and chemistry. The circumstances in which he is placed have led him to adopt, in many cases, far more sensible modes of experimenting, and easier plans of effecting his purposes, than are commonly employed by lecturers. But we chiefly mention the circumstance, in the hope that it may be the means of bringing Mr. Davidson acquainted with the Society in Sunderland for preventing Accidents in Coal Mines; as Dr. Watt informs us, that among other contrivances he has invented a mode of clearing mines from deleterious gases, which for efficacy, facility, and cheapness, is preferable to any of the methods hitherto employed.

ELECTRICITY.

Monsieur de Nelis, an indefatigable inquirer into the phænomena of electricity on the continent, having favoured the Editor of the Philosophical Magazine with several valuable communications, accompanied by various specimens of electrical results; Mr. Singer, the eminent lecturer in electricity and Galvanism, has kindly undertaken to repeat the experiments of M. Nelis, and to lay an account of them before the readers of the Philosophical Magazine. In the mean time it may not be amiss to state generally, that M. de Nelis has directed his attention to the subject of electricity for upwards of fifteen years; and he conceives that all his experiments demonstrate the leading principle of Franklin's theory, namely, the existence of a single

a single electric fluid condensed in positive bodies, and rarefied in those which are negative.

Mr. Singer, in a communication alluding to the above experiments, thus expresses himself: "A great number of your readers are certainly not yet acquainted with these experiments, which have excited considerable attention on the continent. I must confess I had regarded them hitherto as of inconsiderable moment; but on examining the results now transmitted to you by M. de Nelis, they appear to me to merit the repetition which he has requested, and I propose to undertake it as soon as the incessant occupation which my lectures, and some researches in Voltaic electricity, at present supply, shall have subsided.

"I will then, sir, with much pleasure submit to your readers an account of these experiments, accompanied by such observations as their repetition may suggest."

Mr. Thomas Forster is about to publish A Sketch of the new Anatomy and Physiology of the Brain and nervous System of Drs. Gall and Spurzheim, considered as comprehending a complete System of Zoonomy. With Observations on its Tendency to the Improvement of Education, the Punishment of Crimes, and the Treatment of Insanity. Reprinted from The Pamphleteer, with Additions.

Another Edition of the London Pharmacopœia, it is said, is now in great forwardness, and will probably be soon before the public. Among the most important of its improvements will be the *emetic tartar*, which, we have been informed, is prepared according to a very elegant and simple process, contrived by Mr. Hume, of Long Acre; in which the common black sulphuret of antimony is in one simple operation decomposed, yielding an oxide suitable for the purpose, which requires merely to be washed before it be submitted to the super tartrate of potash.

A correspondent, H. G., desires us to state, that every vulgar fraction is in its lowest terms that is not divisible by the difference between its numerator and denominator, or by some submultiple of that difference.

LECTURES.

Middlesex Hospital.—Dr. Merriman's next Course of Lectures, on Midwifery and the Diseases of Women and Children, will be delivered at the above Hospital during the months of April and May. The introductory Lecture will be read on Monday, April the 10th, at Half past Ten o'clock.

LIST

LIST OF PATENTS FOR NEW INVENTIONS.

To James Miller, of Liverpool, distiller, for certain improvements in the construction of stills, furnaces, chimneys, and other apparatus connected with the art of distillation.—28th January 1815.—6 months.

To John Wood, of Manchester, for certain improvements in machinery used for preparing and spinning cotton, wool, and various other articles.—4th Feb.—6 months.

To Joseph Taylor and Peter Taylor, both of Manchester, for a certain improvement in a loom to be used in weaving cotton, worsted, silk, or other cloth.—4th Feb.—2 months.

To James Thomson, of Primrose Hill, in the county of Lancaster, calico printer, for certain improvements in the process of printing cloth made of cotton or linen, or both.—4th Feb.—2 months.

To William Griffith, of Giltspur Street, London, for an improved toast-stand.—7th Feb.—6 months.

To Richard Jones Tomlinson, of Bristol, for certain improvements in the method of framing, constructing, or putting together the roofs of buildings, or the parts thereof.—9th Feb.—2 months.

To William Moult, of Bedford Square, in the county of Middlesex, for his mode of evaporation and sublimation.—13th Feb. —6 months.

To Jonah Dyer, of Woottonunderedge, for his improved frame or machine for shearing of woollen cloth.—21st Feb.—2 months.

To Joseph Burrell, of Thetford, for his support and safe-guard in getting in and out of chaises and other two-wheeled carriages. —21st Feb.—2 months.

To Ralph Dodds and George Stephenson, of Killingsworth in the county of Northumberland, engineers, for various improvements in the construction of locomotive engines.—28th Feb.—2 months.

To Samuel Brown, of Mark Lane, London, commander in our Royal Navy, for his rudder and certain apparatus connected therewith, for governing ships and vessels of all descriptions with much more certainty and effect, and for producing various advantages not hitherto practised or known.—28th Feb.—2 mo.

To Dudley Adams, of Fleet Street, London, mathematical instrument maker, for certain improvements in the construction of paper vellum tubes for telescopes and other optical parts of telescopes.—7th March.—2 months.

To Thomas Deakin, of Ludgate Hill, in the city of London, furnishing ironmonger, for his portable kitchen.—7th March.—6 months.

To

To William Mitchell, of Glasgow, and John Lawton, of King Street, Snow Hill, London, for their improved lock and key.—7th March.—6 months.

To William Wood, of Shadwell, shipwright, for his material or materials, and the application thereof to the more effectually making water-tight and sea-worthy, ships and all other vessels, which he denominates "adhesive felt."—9th March.—6 months.

To Elizabeth Beveridge, of Hatton Garden, London, for an improved bedstead.—14th March.—2 months.

To John Mills, of Holywell Street, and St. Clement's Church-yard, Strand, for his improved elastic stays for women and children, and also to give relief to women in a state of pregnancy.—14th March.—2 months.

To Robert Dickinson, of Great Queen Street, Lincoln's Inn Fields, in the county of Middlesex, esq. for certain improvements in the making or fabrication of sundry tools, implements, or articles used in various arts or manipulations, or the ordinary occasions of life.—14th March.—6 months.

To William Bell, of Edinburgh, writer to the signet, for certain improvements in the apparatus for copying manuscripts or other writings or designs.—14th March.—2 months.

To Jonathan Ridgeway, of Manchester, for his method of casting and fixing at the same time metallic types on the surface of metallic cylinders or metallic rollers, or any cylinders or rollers having metallic surfaces, or on blocks of metal, or on blocks having metallic surfaces, or on flat metallic plates for the purpose of printing patterns on cloth made of cotton, or linen, or both.—14th March.—2 months.

To Thomas Potts, of Batchworth Mills, Rickmansworth, for his new mode or means of combining and applying principles already known to the purpose of producing pure fresh warm air, and of such mode or means of combination and application of principles already known to such purposes as aforesaid.—14th March.—6 months.

To Henry Houldworth, of Anderston, near Glasgow, civil engineer, for his new method of discharging the air, or air and condensed steam, from pipes used for the conveyance of steam for the purposes of heating buildings or other places.—18th March.—2 months.

METEORO-

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand,

For March 1815.

Days of Month.		Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Feb.	25	47	52	47	30·01	12	Cloudy
	26	47	47	38	·07	7	Stormy
	27	35	50	39	·45	16	Fair
	28	32	47	40	·32	15	Fair
March	1	40	55	46	·18	12	Fair
	2	46	45	40	·12	0	Rain
	3	39	52	47	·19	10	Cloudy
	4	46	53	46	·20	12	Cloudy
	5	47	53	44	·19	10	Cloudy
	6	44	54	44	·16	18	Fair
	7	47	50	47	29·85	16	Cloudy
	8	47	49	40	·40	0	Rain
	9	40	48	40	·30	7	Cloudy
	10	40	47	35	·25	13	Fair
	11	33	47	36	·42	17	Fair
	12	37	47	45	·21	0	Rain
	13	42	48	40	28·90	0	Stormy
	14	41	47	40	29·80	0	Stormy
	15	40	50	50	·84	0	Rain
	16	43	57	46	·98	22	Fair
	17	47	58	46	·96	25	Fair
	18	47	57	50	30·00	15	Showery
	19	50	57	50	29·96	16	Cloudy
	20	51	57	49	·95	16	Cloudy
	21	50	58	50	·70	26	Fair
	22	50	57	51	·58	14	Stormy
	23	51	50	42	·25	11	Stormy
	24	47	54	50	·40	12	Stormy
	25	50	53	40	·38	21	Fair
	26	43	50	49	·78	14	Stormy

N.B. The Barometer's height is taken at one o'clock.

XLIII. *On the late Plague at Malta.* By Joseph Skinner, *Esq. late Surgeon to Prisoners of War at Malta.*

In what way this terrible scourge was introduced into the island of Malta, is not to the present purpose; but it is to be trusted that for the future, whenever infection shall be known to prevail on shipboard, and to hover about the vicinity of any of the British settlements, there will be but one sentiment relative to the precautionary measures to be adopted. At the time of this visitation Malta was substantially a British colony, although not declared so: the Maltese, however, were not brought into that state which could prevent the spreading of the contagion at the onset. They are a people who, being strongly attached to their native soil, seldom migrate to make observations elsewhere. The history of the plague, as referring to their own island, was too remote to serve them as a guide; and it was not until many of them had been swept away, that they could be made to believe in the real existence of the calamity, which they mistook for an ordinary disease. In the interim, bigotry in all the ranks was combined with the cupidity natural to the lower class of the Maltese. The churches were as diligently attended as if no mischief could result from the contact of many individuals; the host was paraded with numerous followers; and visits paid to the infected, without scruple and without hesitation, by their relatives and friends. In this way, and by the concealment of pestilential effects, the disease not only spread through the city of Valletta, but was introduced into several of the casals or villages, before the strong hand of power could stay its progress.

When these facts are considered, it will not appear surprising that this dreadful scourge, which for several months devastated the island of Malta, and was subsequently introduced into Gozo, was principally confined to the indigenous inhabitants of those islands. Few of the Turks or Greeks resident at Valletta, the capital, were attacked, if we except those of the lower class of Greeks, who, in common with the felons, were engaged in the hazardous employments which the exigencies of the occasion required. Other foreigners were equally exempt; and the British peculiarly so. It was indeed a matter of fearful wonder to the Maltese, who regard all protestants as heretics*, and therefore

* At Valletta, some years ago, during the funeral procession of the late Admiral Sir A. J. Ball, the civil commissioner, whose excellent qualities had endeared him to every class of the inhabitants; a Maltese was heard to say to another, "What a pity that so good a man should go to hell!" During his fatal illness the Bishop of Malta paid Sir Alexander several visits, the drift of which may be easily conceived. To have gained over such a proselyte would have been a great triumph.

the fittest subjects for Almighty vengeance, that of all others they should have been most favoured under this visitation. With the exception of two females, who had been abandoned by the military, and were sent, with the charge of children, to the spot destined to receive the population of the infected quarters of Valletta;—of the wife of a serjeant of artillery who resided at Floriana, when the disease raged with violence in that suburb;—and of the last fatal case at Gozo, which will be touched on hereafter, the writer does not recollect any British who were victims to the disease, the soldiery of one of the military corps excepted. At the breaking out of the plague at Valletta, many of the British families resident there sought refuge in the country. It is highly to the honour of those who remained to face the danger, and who were not riveted to the spot by public employments, that they were most instrumental in checking the propagation of the contagion, displaying, on every occasion in which they volunteered their services to the Government, an heroic courage and a zeal beyond all praise!

Fort Manoel, situated opposite to Valletta, on the other side of the Quarantaine creek, was at the beginning the receptacle of the sick, who were, together with the remaining tenants of infected houses, poured in from the capital in proportion as the disease gained ground, and before pest-hospitals could be prepared for their reception. Huts were constructed in the sequel, to the end that the entire population of the parts of the capital most infected might be cleared. These were, the Mandragio, a low, obscure, and crowded quarter chiefly inhabited by market people, and another low spot in the vicinity of Fort St. Elmo. Before these measures could be adopted, the population of the buildings of Fort Manoel became overcharged; and here a particular symptom of plague, which does not appear to have been noticed by any writer on the subject, presented itself. Owing to the stimulating quality of the pestilential virus, a furor, or what may be better termed a *satyriasis*, was induced in both the sexes, which required all the vigilance of the attendants to separate them, and that under the most loathsome circumstances of the disease. It is possible that this fact may lead hereafter to a better knowledge of the specific quality of the contagious matter. Subtle and permeating in its nature, in attacking the vital energies, it produces that peculiar excitement in the system, which has been hitherto almost exclusively confined to the pathology of maniacal cases, and of uterine affections in females.

Several of the Maltese medical practitioners, or as they style themselves professors, were selected by the Government, and allowed handsome salaries, to administer to the sick who had been removed to Fort Manoel, and to watch over the safety of Valletta,

Valletta, by visiting the houses where a suspicion of infection was entertained; at the same time that the most prudent measures were adopted to prevent the concealment of disease. For this purpose constant visits were paid in the respective districts into which the city was divided. These districts were separated by barriers, to the end that each of them might be the more closely watched, and all unnecessary intercourse between the inhabitants prevented. The latter were, indeed, with the exception of such as could render themselves useful abroad, confined to their homes, the supplies of provisions being carried round by persons appointed for that purpose. Proclamations were issued, pointing out the precautions they had to observe in receiving these supplies, &c. In short, every salutary and restraining measure which the public safety required was promptly taken by the Government.

Having adverted to the Maltese practitioners, whose exertions for the relief of their fellow citizens were thus called forth, and liberally recompensed, some account of their practice—of the curative means they employed—may be expected. To come, however, at any precise knowledge of the treatment they pursued, baffled all the ingenuity of inquiry. It is not but that they were sufficiently accomplished, in point of study, for the task they undertook. Several of them may, on the other hand, be cited as highly accomplished in their profession *; but it was not an easy task for them to subdue the constitutional timidity under which they laboured, on the sudden appearance of a contagious distemper with which they were only theoretically acquainted. To follow up any thing like a regular method of cure, required a free and unrestrained communication with the patients under their care, which they could not maintain unless exempted from the panic that was spread around them. History records that on the breaking out of the great plague of London in 1665, among other instances of an heroic contempt of danger—of an ardent zeal in the cause of humanity, which could not be abated by any consideration of personal safety—a Doctor Sayer declared that, braving every risk, he would attend indiscriminately the rich and the poor. He persevered until the last, and escaped the contagion †. Nothing of this sort was to be heard of at Malta: it would in truth appear, that every idea of systematic treatment yielded to the powerful impressions of fear, as well at the onset as when the disease raged with the greatest violence. Even

* The Maltese public have to regret the loss of Doctor Gravagna, a sensible and judicious practitioner, of very amiable manners. He died in the time of the plague, but it is uncertain whether he fell a victim to the disease.

† Among the precautions with which he armed himself, it was his custom to take a copious draught of Madeira wine on leaving his house, and another on commencing his rounds.

 when,

when, towards the close, the symptoms became milder, and the attacks less frequent, the same timid caution—the same dread of approaching the patient, so as to be enabled to direct a particular attention to the symptoms of his case—was to be noticed*.

In aid of the Maltese practitioners, several Turks and Greeks were, at the instance of the Government, sent to Malta from Smyrna. They are said to have been useful, not on account of any particular knowledge they possessed of the management of plague, but because they had been so accustomed to it, and in a manner seasoned, that they braved its attacks in their attendance on the sick.

The attempts at plague inoculation were founded on the commonly received opinion, that those who have had the disease (and it is presumable that this was the case with several, if not with all the above individuals†,) are not again liable to infection. They may be less susceptible than others: but a Frenchman who had been servant in an English family at Valletta, and who, having caught the plague, had been sent to Fort Manoel for cure, had, during his stay there, two subsequent attacks, from the latter of which, although a sharp one, he recovered perfectly.

A Neapolitan physician, who had seen much of the plague in Turkey and Greece, was among the boldest of the practitioners. He did not hesitate to approach the sick, and to treat them with freedom. It was his practice to cauterize the pestilential tumors, and towards the close of the disease, when the inflammatory symptoms had subsided, to make a liberal use of cordials and alexipharmics.

It remains now to pay a well merited compliment to the British medical staff, whose exertions were unfortunately required by the breaking out of the plague in the third garrison battalion, and likewise in De Rolle's regiment, consisting entirely of foreigners.

The military pest-hospital was under the management and direction of Ralph Green, Esq. inspector-general of hospitals.

* At Valletta, in a part of the fortifications, huts were erected for such of the prisoners of war as had been released on the condition of their undertaking the tasks of sweeping the streets, white-washing infected houses, &c. in the event of their being seized by plague. A suspicious case having occurred among them, the Maltese practitioner on duty kept the respectful distance of sixteen paces from his patient. It is true that he was provided with glasses; but how far, with this interval between the parties, they helped him in his timid inquiries, is uncertain.

† Smyrna is said to be never entirely free from plague, more especially in the district occupied by the Turks. In that city, at Alexandria, and indeed wherever the disease is familiar, those whose temperament has enabled them to resist an attack, or who have escaped the contagion under circumstances of the greatest exposure, are selected to administer to the sick. There may be habits in which there is not the slightest susceptibility to receive the infection, as in the example of the natural small-pox.

The

The patients had every advantage which could be afforded them by skill and science, aided by that courageous zeal which spurns every idea of personal safety, when an imperious duty is to be performed*. Accordingly, the proportion of recoveries was much greater than that which the Maltese practice has to record. Not any of the attendants took the infection. They were made to bathe, and employed frictions of warm oil, as did also the military on duty. In addition to this latter preservative, recourse was had to cold ablutions of vinegar and water.

With the symptoms by which the plague is characterized, it presents others, in its attack, similar to those of the *endemial causus*, or bilious remitting fever, commonly styled in the Mediterranean the fever of the country. At the commencement it therefore requires the same antiphlogistic treatment, by bleeding and other evacuants, to diminish the powerful determination to the brain, the oppression of which is among the earliest of the symptoms, accompanied by stupor and delirium. On the subsidence of the inflammatory symptoms, the case having taken a favourable turn, the method of cure is in either fever the same, independently of what belongs to the tumours and other characteristic signs of plague. The symptoms, as they have latterly presented themselves, and the treatment which appears to have been most successful, are described by Ulstadius, who wrote on plague so far back as the commencement of the sixteenth century †; and by Sennertus, who collected all the authorities up to the middle of the seventeenth.

Several persons at Malta, and among others a British merchant, asserted that they could distinguish a glare, a peculiar wildness of the eyes, before the individual himself in whom it was perceived, was sensible of the attack of plague, and while he was still following his usual occupations. The writer had to witness the effect of a sudden attack in a Maltese, who had proceeded, apparently in good health, as far as the Conservatory square at Valletta, when his progress was in a moment arrested. He had just sufficient strength to maintain himself in an erect posture; but was obliged to be supported on either side when

* The following melancholy fact is a proof, among others, that an ardent desire to procure information with a view to benefit mankind, will sometimes carry an individual beyond the prescribed limit of his duty. Dr. MacAdam, physician to the forces, was sent by the governor of Malta to Gozo, to direct the means to be employed in the case of the plague breaking out among the military stationed there. He was particularly enjoined not to incur any personal risk, his being a task of mere superintendance. His anxiety, however, to acquire a precise knowledge of the nature of the disease, led him to pay frequent visits to the pest-hospital, where at length he caught the infection, and was the last victim of the scourge which so long ravaged the islands of Malta and Gozo.

† His Treatise appeared in 1526.

taken to the bier on which he was conveyed to the pest-hospital. His eyes were downcast, his countenance pallid, and there was an expression of anxious terror which sufficiently explained the quality of the attack.

Notwithstanding all that had been advanced in their favour by various writers, and more especially by Messrs. Baldwin and Thornton; and in spite of the example of the oil-carriers in Africa, who are represented as enjoying an immunity from plague; the frictions of warm oil had not, on the breaking out of that disease at Malta, all the credit which an after experience showed them to deserve. This will not appear surprising, when it is considered that several late medical writers have spoken with considerable hesitation on the subject. After the numerous trials made at Valletta, and elsewhere in Malta, these frictions, if properly applied*, may be pronounced to be an almost certain prophylactic. It is a justice due to a very intelligent young man, Mr. Thornton, assistant deputy paymaster to the forces, and nephew to our minister of that name, to state that, with his uncle's book in his hand, he was the first strenuously to recommend their employment, at the breaking out of the contagion. Among those who wrought zealously in this cause, Mr. Matthew Fletcher, a British merchant, was foremost and indefatigable. Whenever a case of plague came to his knowledge, he hastened to the spot, beseeching the inmates of the dwelling, by whom a free communication with the infected individual had been kept up, to have instant recourse to the frictions, and supplying the means where these were deficient. The happy result was, that, on the plague ceasing, he was possessed of a long list of the cases he had recorded, in not one of which a failure was to be found; at the same time that, where this preservative was not employed, it was usual to see the disease spread from one individual to another who herded together, until the whole were swept away. A few instances of the beneficial effects of the oil frictions, as tried at Valletta, will suffice, so as to banish all scepticism on the subject. In a family consisting of seven individuals and a female servant, the father and eldest son, who had both been assiduous in attending the host, and had besides visited a relation labouring under plague, were attacked, and died pretty nearly at the same time. On the first alarm the frictions were employed by the rest of the family, and all escaped, although they had communicated freely with the unfortunate victims of the disease. A French cutler, who had the same number of

* Every part of the body having been well cleansed with water, or with vinegar and water, the frictions are applied with a sponge, as warm as they can well be borne, the eyes being closed to protect them from irritation. This is repeated at least twice a week, wearing the same linen between the frictions.

children,

children, had married his eldest daughter to a person who took the plague of the next door neighbour, as did likewise the second daughter of the family. The two sickened about the same time, and these also were fatal cases: but the remainder of the family, who had had recourse to the oil frictions as soon as the nature of the attacks was ascertained, escaped, without excepting the wife of the young man, who was then in the middle of her pregnancy, and who attended her husband during the few hours he survived the attack. A Maltese with a large family took a sick brother into his house, not suspecting that he laboured under the plague, and paid him unhesitatingly every affectionate attention. The instant this came to the knowledge of Mr. Iliff, apothecary to the forces, to whom the Maltese in question had formerly been a servant, the oil frictions were sent in and employed. The infected individual died, but the whole of the family escaped. It is needless to cite any other cases, although many similar ones might be adduced. The confidence of the Government of Malta, in the efficacy of these frictions, was at length so great, that a shed was erected at each of the barriers of Valletta, for the purpose of administering them to the guards stationed there, and to the market people and others whose avocations kept them abroad.

Alexis, the Piedmontese, who travelled every where in search of secrets, has published a variety of receipts for plague, into the composition of several of which storax enters. The writer was solicitous to make a trial of this substance, which unluckily was not to be found at Malta, either in the concrete state in which it is called storax *calamita*, or in its liquid state*. It was certainly, as the event proved, deserving such an essay, and was susceptible of various modifications in its use. What has been sanctioned by a long experience deserves credit, unless there be incontrovertible evidence to prove that the notion originally entertained of its effiacy was founded in error. A Turk, whose knowledge of the subject was by no means limited, distributed among his friends at Valletta lumps of a black substance resembling shoemaker's wax, which he had brought with him from Constantinople, and in the composition of which storax was the principal ingredient. These were either to be carried on a stick and smelt to from time to time, or kept in the hand and constantly moulded, to the end, no doubt, that a portion of the substance might adhere to the fingers. They were sought after with avidity. Now, it is to be observed that both the Seraglio cakes, and a particular description of beads of a great price in

* It is with this lattter substance that the fire-eaters, as they term themselves, anoint the tongue and fauces to protect them from the effect of caloric.

Turkey, are of a similar composition. It is therefore to be presumed, that those who brought them into use had something more in view than to supply an ornament in the case of the former, and in that of the latter, an object of pastime, in which light the generality of the Turks, who are in the constant habit of twirling them about with the fingers, regard beads.

Common tar, a bituminous substance which may be considered as in some degree analogous to storax, although it does not possess its peculiar fragrance, was employed by a Greek whose very hazardous task it was to bury the dead. With this substance he kept his hands and arms anointed. He was pointed out to the writer by Mr. Thomas, garrison surgeon, and acting superintendant of health, as having officiated with impunity in this way during the whole of the time that the plague raged; while the greater part of those who were similarly employed had been swept off by the contagion. In this instance it would appear, that the pores of the parts exposed to contact with the dead bodies were sheathed by the tar, so as to prevent the absorption of the plague matter; but in an old work entitled "The English Housewife," described by Beloe in his Anecdotes of literature and scarce books, another application is made of that substance as a preservative against plague. It is recommended "to smell to a *nosegay* made of the tasselled end of a ship-rope," that is, of a tarred rope. Here something is implied of a specific quality of the tar, as a plague preventative; and this application of it agrees with that of the storax as employed by the Turks.

JOSEPH SKINNER,
Late Surgeon to Prisoners of War at Malta.

London, March 30, 1815.

XLIV. *Some Account of the Island of Teneriffe. By the Hon.* HENRY GREY BENNET, *M.P. F.R.S. Pres. Geol. Society**.

THE island of Teneriffe is the principal island of the seven in the Western ocean, that are called generally by the name of the Canaries. It lies north-east by south-west, and is in length from the *Punta del Hidalgo* to the *Montana Roxa*, its northern and southern extremities, about 70 English miles; its greatest breadth not exceeding 30. The superficies may be considered as containing 80 square leagues.

The island narrows at its north-eastern and widens considerably at its south-western extremity. About the centre of the latter, or, perhaps, to describe more accurately, to the westward of the central point, is the mountain called by the Spaniards *El Pico*

* From the Geological Transactions, vol. ii.

di

di Tiéde, but better known by the name of the *Peak of Teneriffe*, and which is the highest land not only in the island, but in all the *Canaries;* the mean of various observations making it 12,500 feet above the level of the sea. It is visible at a great distance; we saw it perfectly distinct thirty-four leagues off by chronometrical observation, when it appeared rising like a cone from the bed of the ocean; and I have heard that it has been clearly distinguished at a distance of 45 leagues.

The rocks and strata of the island of Teneriffe are wholly volcanic; a long chain of mountains, which may be termed the central chain, traverses the island from the foot of the second region of the peak sloping down on the eastern, western, and northern sides, to the sea. Towards the south, or more properly the SSW. the mountains are nearly perpendicular; and, though broken into ridges and occasionally separated by deep ravines that are cut transversely as well as longitudinally, there are none of those plains nor that gradual declination of strata that the south-eastern and north-western sides of the island exhibit.

From the *Barranco Seco*, in the neighbourhood of *Santa Cruz*, to the northerly point called *Punta del Hidalgo*, a series of steep and abrupt mountains form headlands to the sea, separated from the central chain by the valley of *Laguna;* these mountains are rugged and peaked, drawn up, if the term may be used, in a column, and are divided by deep ravines. The sides of these mountains are steep, being in many places cut nearly perpendicular to the horizon, and are all composed of lava generally of the basaltic formation, mixed with beds of tufa and pumice. From *Hidalgo* point to that of *Teno*, the most westerly point of the island, the strata vary from beds of pumice and decomposed lava and ash, which form the plains of *Laguna Ticaronte* and *Songal*, to streams and currents and headlands of lava similar to those of the *Burranço Hundo*, *San Ursula*, *Las Horcas*, and *Las Guanchas*. The slope from the central chain is here gradual, intersected by ravines and streams of lava. The soil, famed for its fertility and which produces the Teneriffe wine, is composed of lava and ash in a state of decomposition. Headlands, some of them from two to three hundred feet in height, project into the sea between *San Ursula* and *Orotava*, forming perpendicular cliffs. At the western extremity of the island from *Punta di Teno* to *Puerto de los Christianos*, the strata rise in a broken ridge to the Peak, the land ascending gradually from *Punta de Teno* by a chain of small peaked hills; the point itself being very low and projecting as a promontory into the sea. The declination of the strata is similar from the Peak to *Puerto de los Christianos*. This south-westerly chain is broken into many abrupt

abrupt ridges, and is cut nearly perpendicular down to the sea. I could not perceive any base or shelf as on the other sides of the Peak, from which the cone arose, but the fall is regular though steep. From *Puerto de los Christianos* to Santa Cruz, comprising the southern and south-eastern sides of the island, the form is similar to that in the vicinity of *Orotava;* but it is barren and desolate, laid waste by streams of lava. In the short space of a few leagues I counted no less than seven cones of extinct volcanoes, and the country is covered with scoria, exhibiting no appearance of culture, and hardly any of vegetation; it is more broken into ravines and more intersected by lava torrents than on any of the other sides of the island. Numerous peaked and conical mountains rise upon the slope of the chain, and the whole country is covered by scoria, and is one continued stream of lava. The *Montana Roxa* itself is a singular example of the dislocation of strata so commonly found in countries of volcanic formation; it is evidently a slip or fall of semi-columnar lava, and slopes into the sea at a highly inclined angle.

The ordinary strata of the island are as follows, reckoning from below upwards: 1st, the porphyritic lava covered by scoria and sometimes by pumice. This lava is composed of hornblende and feldspar, and contains no other substance. The next stratum graduates into what the Spaniards call *roccaverde* or greenstone, and is composed of feldspar and hornblende; upon this is generally a thick stratum of pumice, and last of all towards the surface is the basaltic lava covered also by tufa and ash. This lava decomposes the soonest. It also contains the greatest variety of extraneous substances, and is sometimes divided by a layer of large crystals of olivine some inches long, and towards the north-east is often intersected by strata of porphyritic slate. These lavas are more earthy and cellular than those which I have had an opportunity of observing elsewhere, yet they contain fewer extraneous substances than those of Ætna and Vesuvius; they are in some places exposed to view in the valleys similar to those of the *Corral* in the island of Madeira. The valley of *Las Guanchas* on the north-west side of the Peak, contains according to M. Escolar* above 100 strata of lava, the one reposing upon the other, at times alternating with pumice and tufa. The depth of these strata varies. M. Escolar has seen one of basaltic lava between 100 and 150 feet in depth in one solid mass, cellular at the surface, but gradually becoming more compact to-

* M Escolar was sent out by the Spanish Government to examine the political, commercial and mineralogical state of the Canaries: he has well performed his task, and it is to be regretted that the situation of his native country has hitherto deprived the public of the interesting facts he is able to communicate.

wards

wards the bottom. This basaltic lava contains olivine and hornblende, and, in the caves on the coast, zeolite. This substance is also found in stalactites and in masses, sometimes in layers spread between the strata and diffused over the rock.

Nodules of chalcedony are sometimes also found; but these substances occur only in the chain of mountains towards the north-east, from the northern extremity of Santa Cruz to the point of Hidalgo.

The lavas of the island are of an endless variety, and the number of streams that have flowed are much beyond all enumeration. The whole surface is either ash, or solid or decomposed lava, which seems again and again to have been perforated by volcanic eruptions; the number of small extinct volcanoes is prodigious, they are to be found in all parts of the island, but the stream that has flowed from even the largest of them, such as the lava of the Peak called *El Mal Pais,* is trifling in comparison with that immense mass of lava mountains which constitute the central chain of the island, and which stretch out as headlands like those of *Las Horcas* and *San Ursula*.

I never found *in situ* those masses of columnar basaltic rock that are so common in the island of Madeira: but in the valley of *Las Esperanzas*, in the chain of hills to the north-eastward of the town of Santa Cruz, they lie scattered about in considerable numbers, and M. Escolar told me that he had seen strata of them to a considerable extent, exhibiting with precision the columnar basaltic form: the modern lavas of the peak are all basaltic, that of 1704 is decidedly so, as well as that of 1798, though not exhibiting any prismatic form. Prisms of basaltic lava are yet found on the peak: I picked up one, though there are no strata of them to be met with. The metals are rare, and afford but little variety: specular and micaceous iron, black and grey manganese are all that have hitherto been discovered. The salts that are so common on Vesuvius, are here seldom met with. Augite is also rare, and mica and leucite, though carefully sought after, have hitherto not been found.

In that part of the island between *Laguna* and *Tacaronte*, where there are few streams of lava, the soil is evidently volcanic. I examined many of the clods that were turned up by the plough, and found them all alike: they contained much strong clay, with crystals of feldspar, olivine, and specular iron. Dr. Gillan, who accompanied Mr. Barrow and Sir G. Staunton, has advanced an opinion, that between *Laguna* and *Matanzos* there are no signs of volcanic formation. That the currents of lava occur but seldom is most true; but the mountains in the vicinity of Laguna are all volcanic, and one has a visible crater: besides, the assertion would prove too much; for it would go to maintain that the

Campagna

Campagna Felice, as well as the plains of Catania, were not created by the ash and pumice eruption of Vesuvius and Ætna. The bed of soil is here very deep. I examined some ravines that the rains had laid open to the depth of 30 or 40 feet: the strata were indurated at the bottom, and resembled the tufa in the vicinity of Naples, and all contained the substances mentioned above. This tufaceous character changes as you ascend the hill that separates Laguna from Santa Cruz; the hill itself, and the whole neighbourhood of the latter city, is one continued stream of lava, hardly at all decomposed, with little or no vegetation; but here and there in the hollows some few stunted plants of the *aloe algarvensis*, and the *cytisus.*

Having given a general account of the island, I shall now attempt to describe the country of the peak, which mountain I ascended on the 16th of September 1810. The road from Puerto Orotava to the city of Orotava is a gradual and easy slope for three or four miles, through a highly cultivated country. The soil is composed of volcanic ash and earth, and to the eastward of the town of Puerto di Orotava are the remains of a recent volcano, the crater and cone being distinctly visible. Leaving the town of Orotava, after a steep ascent of about an hour through a deep ravine, we quitted the cultivated part of the slope or valley and entered into a forest of chesnuts; the trees are here of a large size. This forest of chesnuts is mixed with the *erica arborea*, or tree heath, which shrub rises to the height of 18 or 20 feet. Some of the stems are as thick as the arm of a man, joined together in bunches or tufts like the common heath. The form of this forest is oblong, it covers the flank of those hills which I have already denominated the central chain, from their summit to half their elevation from the plain. The soil here is deep, and formed of decomposed lava, small ash, and pumice. I examined several channels in the strata or ravines worn by the rains, and there was no appearance of any other rock. Leaving this forest, the track passes over a series of green hills which we traversed in about two hours, and at last halted to water our mules at a spot called *El barranco del pino de la meruenda*, where there is a small spring of bad and brackish water issuing from a lava rock. The ravine is of considerable depth. After the vegetable earth, which is two or three feet deep, a layer of tufa succeeds, which is followed by a lava of a greyish-blue colour, 30 or 40 feet in depth. It is compact, contains olivine, and the strata lap over each other, but show no appearance of columnar formation. The range of green hills extends a mile or two further, the soil shallowing by degrees, more lava and scoria showing themselves on the surface, the ravines or channels, worn by the rains, becoming more common, the trees and shrubs gradually dwindling

dwindling in size, and of them all the Spanish broom alone at length covers the ground. Leaving behind us this range of green hills, the track still ascending leads for several hours across a steep and difficult mass of lava rock, broken here and there into strange and fantastic forms, worn into deep ravines, and scantily covered in places by a thin layer of yellow pumice. The surface of the country, for miles and miles around, is one continuous stream of lava; the rents or ravines of which seem to be formed partly by the torrents from the hills flowing for so many ages, and partly from that tendency, characteristic of a lava current, to keep itself up in embankments, and in its cooling process to open out into those hollows which I have uniformly found in every eruption of lava that I have had an opportunity of examining. This lava is cellular beyond any I have ever seen, is of a clayey earthy porphyritic composition, and contains few, if any, pieces of olivine, though here and there felspar in a semicrystallized form. As we proceeded on our road, the hills on our left, though broken at times in deep ravines, gradually rose in height till the summits were lost in those of the central chain, while on our right we were rapidly gaining an elevation above the lower range of the peak. This range forms one flank of the plain or valley of *Orotava*, stretching from south-east to north-west, and is broken into steep precipices, cut down in some places perpendicular to the horizon, and called *Las Horcas;* it joins the central chain at the high elevation of the pumice plains, sweeps down the side of the valley, and forms a headland near 200 feet high projecting into the sea, some miles from *Orotava:* we traversed this country an hour or two, till we reached the point of intersection of *las Horcas* with the plains of pumice. On the road are several small conical hills or mouths of extinct volcanoes, the decomposed lava on the edges of these craters having a strong red ochreous tint; by degrees the lava becomes more and more covered by a small ash, and the masses or heaps of pumice gradually increase, till the surface is completely concealed. At length an immense undulated plain spreads itself like a fan, on all sides, nearly as far as the eye can reach, and this plain is bounded on the west south-west, and south south-west, by the regions of the peak; and on the east and north-east by a range of steep perpendicular precipices and mountains, many leagues in circumference, called by the Spaniards *Las Faldas*. M. Escolar informed me that the wall could be traced for many leagues, the whole circumference of which evidently formed the side of an immense crater. This tract, called *Las Canales*, contains, according to the same authority, twelve square leagues. As we entered this plain from the south-west, there

are

are to be seen several declivities of lava and strata, broken inwards towards the plain, and evidently a continuation of the above-mentioned line of wall and the remains of the original crater. There is here no appearance of columnar formation, the lava being earthy and porphyritic: this continuity of wall, at present so easy to be traced, may be considered as forming the sides of one immense crater, from which perhaps originally the lavas of the island flowed, which might have thrown up the cone of the peak, and covered these wide-spreading plains or *clamuras* with the deep beds of ashes and pumice. On this plain or desert, for we had long left all show of vegetation, except a few stunted plants of Spanish broom, a sensible change was felt in the atmosphere; the wind was keen and sharp, and the climate like that of England in the months of autumn. All here was sad, silent, and solitary. We saw at a distance the fertile plains on the coast, lying as it were under our feet, and affording a cheerful contrast to the scenes of desolation with which we were surrounded; we were already 7 or 8000 feet above the level of the sea, and had reached the bottom of the second region of the peak. Immense masses of lava, some of them many hundred tons in weight, lie scattered on these pumice plains. Some are broken by their fall, and all wear the appearance of having been projected by volcanic force. Their composition is uniformly porphyritic, with large masses of feldspar; the whole compact and heavy, and bearing no resemblance to the earthy lava we had seen in such abundance prior to our entering these pumice plains. Many of these masses are completely vitrified, while others only show marks of incipient vitrification; but from their site and fracture, from the insulated state in which they lie, from there being no appearance of lava in a stream, from the pumice bed being very deep, (and in one place I saw it exposed to a depth of between 20 and 30 feet) from all these facts taken together, there can be little doubt that these masses were thrown out of the mountain when that lava flowed, which is of similar substance, and which is called by the Spaniards *El Mal Pais*.

Having reached the end of the plain we found ourselves at the bottom of a steep hill, at the foot of which is a mass or current of lava which has flowed from the higher regions of the peak, and which constitutes the eastern branch of the lava of *Mal Pais*. We began to ascend this steep and rapid part of the mountain, which is composed of a small white or yellowish ash mixed with masses of pumice and fragments of lava similar to that found in the plains, of which several small pieces that I picked up were in a state of vitrification. After a laborious not to say hazardous ascent of about an hour, the pumice and ash giving way and the

mule

mule sinking knee deep at each step, we arrived at about five in the afternoon at the other extremity of the stream of lava, which descending from the summit of the second region of the peak divides at the foot of the cone into two branches, the one running to the north-east and the other to the north north-west: at the extremity of this latter are several immense blocks or masses of lava which bear the name of *La Estancia di los Ingleses*, and are rocks, not caves as has been stated by some writers. It was here we were to pass the night: so, lighting a fire made of the dry branches of the Spanish broom, and stretching part of a sail over a portion of the rock, we ate our dinner and laid ourselves down to sleep. I however passed the best part of the night by the fire, the weather being piercing cold: as I stood by the fire the view all around me was wild and terrific; the moon rose about ten at night, and though in her third quarter gave sufficient light to show the waste and wilderness by which we were surrounded: the peak and the upper regions which we had yet to ascend towered awfully above our heads, while below, the mountains that had appeared of such a height in the morning, and had cost us a day's labour to climb, lay stretched as plains at our feet: from the uncommon rarity of the atmosphere the whole vault of heaven appeared studded with innumerable stars, while the valleys of Orotava were hidden from our view by a thin veil of light fleecy clouds, that floated far beneath the elevated spot we had chosen for our resting-place: the solemn stillness of the night was only interrupted by the crackling of the fire round which we stood, and by the whistling of the wind, which coming in hollow gusts from the mountain resembled the roar of distant cannon.

Between two and three in the morning we resumed on foot our ascent of the same pumice mountain, the lower part of which we had climbed on horse-back the preceding evening: the ascent became however much more rapid and difficult, our feet sinking deep in the ashes at every step. From the uncommon sharpness of the acclivity we were obliged to stop often to take breath; after several halts we at last reached the head of the pumice hill at its point of intersection with the two streams of lava, the direction of which I have before described. This is the commencement of that division of the mountain called *El Mal Pais*. After resting some short time here, we began to climb the stream of lava, stepping from mass to mass: the ascent is steep, painful and hazardous; in some places the stream of lava is heaped up in dykes or embankments, and we were often obliged to clamber over them as one ascends a steep wall. This lava is of the same porphyritic appearance as the masses we found in the plains; it is not covered with a thick scoria, and seems never to have been

in

in a very fluid state, but to have rolled along in large masses. The feldspar is crystallized in the lava itself, which is slightly cellular at its surface; yet though I searched carefully I was unable to discover any extraneous substance. The whole composition of the stream seems to be felspar imbedded in a brown clayey paste, remarkably hard, of a close texture and heavy: judging from the sharp declivity of the mountain, it appears surprising that the lava should have flowed so short a distance; as it does not exceed two and a half or three miles from the base of the cone to the point of union with the pumice hill. The mass of lava as well as its depth is prodigious; M. Escolar told me that its greatest breadth was above two miles, its depth it is not easy to determine: there are however several ravines or valleys in the course of the stream, some of which may be from 60 to 100 feet deep. The fusion of the mass does not appear to have been perfect; it is very earthy, and though vitrified pieces are found, there is no general appearance of vitrification: there are some pieces that exhibit an union with the pumice and the gradation from the stony structure to the vitrified, and thence to pumice. Immense heaps of this latter lie scattered on the surface of the lava, some of them containing large crystals of felspar, which abounds in, or more properly forms the constituent part of, the lava of the *Mal Pais*.

We halted several times during the ascent, and at last reached a spot called La Cueva, one of the numerous caves that are found on the sides of the mountain: this is the largest of them, and is filled with snow and the most delicious water, which was just at the point of congelation: the descent into it is difficult, it being thirty or forty feet deep. One of our party let himself down by a rope: he could not see the extent of the cave, but the guides declared it to be 300 feet in length and to contain thirty or forty feet of water in depth: the roof and sides are composed of a fine stalactitic lava similar to that found on Vesuvius, and it is of the same nature as that which flowed on the surface. We rested here about half an hour, during which we had an opportunity of observing the rising of the sun, and that singular and rapid change of night into day, the consequence of almost an entire absence of twilight. As we ascended the north-east side of the mountain this view was strikingly beautiful: at first there appeared a bright streak of red on the horizon, which gradually spread itself, lighting up the heavens by degrees, and growing brighter and brighter till at last the sun burst forth from the bed of the ocean, gilding as it rose the mountains of *Teneriffe* and those of the great *Canary*: in a short time the whole country to the eastward lay spread out as a map, the great Canary was easily to be distinguished; and its rugged and mountainous character, similar

milar to that of the other islands, became visible to the naked eye. The cold at this time was intense, the wind keen and strong, and the thermometer sunk to 32 degrees: after a short though rapid ascent we reached the summit of the second stage of the mountain, we passed over a small plain of white pumice on which were spread masses of lava, and at length arrived at the foot of the cone. This division of the mountain forms what is generally termed the *Peak of Teneriffe;* it resembles the present crater of Vesuvius, with this difference, however, that while the surface of that mountain is composed of a black cinder or ash, the superficies of this appears to be a deposit of pumice of a white colour, of scoria and of lava, with here and there considerable masses that were probably thrown out when the volcano was in action. Towards the north-west on the right hand of our ascent, there is a small current of lava showing itself above the pumice, the composition of which is similar to that at the bottom, though of a redder tinge; it is broken on the surface, and is in a rapid state of decomposition. Numerous small cavities on the side of the mountain emitted vapour with considerable heat. Here begins, in my opinion, the only fatiguing part of the ascent; the steepness of the cone is excessive, at each step our feet sunk into the ash, and large masses of pumice and lava rolled down from above; we were all bruised, and our feet and legs were cut, but none materially hurt: at last we surmounted all difficulties, and seated ourselves on the highest ridge of the mountain. This uppermost region does not appear to contain in superficies more than an acre and an half; it is composed of a lava similar to that on its sides, though decomposed and changed white or grey by the action of the sulphurous acid: this acre and an half is itself a small crater, the walls of which are the different points on which we sat, and are plainly visible from below. Within, the lava is in the most rapid state of decomposition; losing its brown colour and shade of red, and acquiring a whitish grey almost the colour of chalk; large masses of sulphur are depositing, which are crystallized in minute though distinct forms; there is also a coating of alum produced by the union of the sulphurous acid with the argil of the lava; the surface is hot to the feet, and the guides said it was dangerous to remain long in one spot: as it was, some of us sunk to our knees in the hot deposit of sulphur: upon striking the ground with the feet the sound is hollow, similar to what is produced by the same impulsion on the craters of *Vesuvius* and *Solfaterra.* I estimate the depth of the crater to be, from the highest ridge to the bottom about 200 feet, forming an easy and gradual descent, the whole being in a state of rapid decomposition, and charged with sulphur, large masses of which are every where depositing. I

 searched

searched in vain for any of the arseniats so common on Vesuvius, nor could I find those siliceous stalactites resembling strung pearls, which are met with in the island of *Ischia*, in the crater of the *Solfaterra*, and in the *Maremma* of *Tuscany*. The sulphur is pure and fine, and is sold for a considerable price at Orotava. We were not able to go all round the walls or exterior summit of the crater, and hence could not distinguish its southern or western declivity; M. Escolar assured me they are similar to, though more rapid than, the side by which we ascended: from this side flowed the basaltic lavas of 1704, and of the last eruption in 1797: this latter stream of lava flowed in a remarkably slow current; for notwithstanding the sharp descent of the mountain, and the length of the lava not exceeding three miles, several days elapsed before it reached the spot where it stopped. How little fluid this lava must have been is evident, when it is remembered that the lava of Vesuvius in 1794, which destroyed Torre del Greco, reached the sea from the bottom of the cone, a distance of eight miles, in little more than six hours. M. Escolar further told me that there is on this south-western side of the peak an ancient lava, at present not at all decomposed, of several miles in length, and in a perfect state of vitrification; the whole of this stream has the appearance of obsidian. All these lavas appear to have flowed from the bottom of the cone, and to have run from its base in the same manner as that of Vesuvius in 1794, the crater of which vomited out ash and pumice, and large pieces of rock, while the current of lava issued from its side. It is not however improbable that the cone itself is of anterior formation to this vitrified lava, as the summit of the Peak is similar to the lava of the *Mal Pais*, and that being porphyritic is considered as of more ancient date than the one above mentioned, which is basaltic.

If one might hazard a conjecture upon a subject where the data are so few, I should be inclined to suspect that the Peak itself, as well as the whole of the country around it which forms its base, were produced by that immense crater called *Las Canales*, the shape and magnitude of which I have before taken notice of when traversing the pumice plains: it is also well worthy of remark, that there is no volcano in action at all to be compared in size of crater to those that are extinct. The ancient crater of Vesuvius is considerably larger than the present, and those in the vicinity of Naples, the eruptions of which probably created that district of Italy, are of enormous extent. The crater of the *Camaldoli* is somewhat more than two leagues in circumference, and the superficies of the *Canales* is estimated at twelve square leagues. These vast craters were probably capable of ejecting from their bosom those stupendous beds of lava,

lava, which being so much more extensive than any that have flowed from more recent eruptions have led some persons to deny the former to be the effects of a central fire. That all the island of Teneriffe was volcanically produced, no man who examines it can have any doubt: and though the smallness of the existing crater of the Peak may lead one to imagine that it alone could not be the effective cause of all the phænomena, yet the innumerable volcanoes on all sides of the island, the appearance of *Las Canales*, and its elevation, are able to account for the extent of the streams and beds of lava, and of the deposits of tufa and pumice, of which the island is composed. Having no data to proceed upon but what is given by the measurement of the eye, it is not easy to determine the magnitude of the cone at its base; one may say at a venture, it is about three miles in circumference, though towards the SSW the descent is much more abrupt, and the plain from which the cone springs not perceptible. The view from the summit is stupendous; we could plainly discover the whole form of the island, and we made out distinctly three or four of the islands, which together are called the Canaries; we could not however see *Lancerotte* or *Fuerteventura*, though we were told that other travellers had distinguished them all.

From this spot the central chain of mountains that runs from south-west to north-east is easily to be distinguished. These with the succession of fertile and woody valleys, commencing from *San Ursula* and ending at Las Horcas, with the long line of precipitous láva rocks that lay on the right of our ascent, and which traverse that part of the island, running from east to west from their point of departure at the *Canales* to where they end in an abrupt headland on the coast, with their forests and villages and vineyards, the port with the shipping in the roads, the towns of Orotava with their spires glittering as the morning sun burst upon them, afforded a cheerful contrast to the streams of lava, the mounds of ash and pumice, and the sulphurated rock on which we had taken our seat. The sensation of extreme height was in fact one of the most extraordinary I ever felt; and though I did not find the pain in my chest arising from the rarity of the atmosphere, near so acute as on the mountains of Switzerland, yet there was a keenness in the air independent of the cold that created no small uneasiness in the lungs. The respiration became short and quick, and repeated halts were found necessary. The idea also of extreme height was to me more determinate and precise than on the mountains of Switzerland; and though the immediate objects of vision were not so numerous, yet as the ascent is more rapid, the declivity sharper, and there is here no mountain like Mont Blanc towering above you, the 12000 feet

above the level of the sea appeared considerably more than a similar elevation above the lake of Geneva. We remained at the summit about three quarters of an hour, our ascent had cost us a labour of four hours, as we left the Estancia at ten minutes before three and reached the top of the Peak before seven; many indeed of our halts were needless, and M. Escolar told me that he had twice ascended to the summit in somewhat less than three hours. Our thermometer, which was graduated to the scale of Fahrenheit, was during our ascent as follows: at *Orotava* at eight in the morning, 74°; at six in the evening at *La Estancia*, 50°; at one in the following morning, 42°; at *La Cueva* at half-past four, 32°; at the bottom of the cone, 36°; at the top of the Peak one hour and a half after sun-rise, 38°. The descent down the cone is difficult from its extreme rapidity, and from the fall of large stones which loosen themselves from the beds of pumice. Having at last scrambled to the bottom, we pursued our march down the other course of the lava, that is to say down its westerly side, having ascended its eastern. The ravines and rents in this stream of lava are deeper and more formidable; the descent into them was always painful and troublesome, often dangerous, in some places we let ourselves down from rock to rock. I can form no opinion why there should be these strange irregularities in the surface of this lava; in places it resembles what sailors term the trough of the sea, and I can compare it to nothing but as if the sea in a storm had by some force become on a sudden stationary, the waves retaining their swell. As we again approached *La Cueva* there is a singular steep valley, the depth of which from its two walls cannot be less than 100 to 150 feet, the lava lying in broken ridges one upon the other similar to the masses of granite rock that time and decay have tumbled down from the top of the Alps; and, except from the scoria or what Milton calls "the Fiery Surge," they in no degree bear the marks of having rolled as a stream of liquid matter. This current like that of the eastward branch has no resemblance to any lavas I have seen elsewhere, it is hardly at all decomposed, full of laminæ of feldspar, the fracture conchoidal, and the texture porphyritic, the colour brown like that of the other branch; it is but slightly cellular, and contains no extraneous substances.

We descended the pumice hill with great rapidity almost at a run, and arrived at La Estancia in little more than two hours. We then mounted our mules, and following the track by which we had ascended the preceding day, we reached about four o'clock the country house of our hospitable friend Mr. Barry.

The difficulties of this enterprise have been much exaggerated: the ascent on foot is not a labour of more than four hours at most, and the whole undertaking not to be compared in point of

of fatigue to what the traveller undergoes who visits the Alps. That the ascent must be hazardous in a storm of hail and snow there can be no doubt, but to cross Salisbury plain may sometimes be dangerous. Yet stripped of poetical terrors and divested of the eloquent description of some writers, there is perhaps no mountain in Europe, the ascent of which does not furnish more difficulties than the Peak of Teneriffe.

XLV. *On Electro-galvanic Agency employed as a Moving Power; with a Description of a Galvanic Clock. By* Francis Ronalds, *Esq. of Hammersmith.*

To Mr. Tilloch.

Sir, — I send you a drawing and description of a contrivance for applying the electricity of M. De Luc's column to the motion of indexes, which arose out of my attempts to facilitate his ingenious method of observing its extraordinary phænomena. If any of the readers of your useful Magazine, by improving upon the method I have stated of regulating the power of the column, or by substituting a better, were to render it subservient to the measurement of time, it would give me great pleasure.

I believe M. De Luc first applied a column of 600 groups to the motion of a small gold bead, suspended by a silken thread between two balls, each of which was connected with the opposite extremity; but not having succeeded by this means in obtaining a vibration sufficiently regular and constant for his observations on its variable action, he abandoned it for one much better adapted to his purpose: he suspended the small gold bead by the finest silver wire from a hook connected with the positive extremity, which hung when unelectrified close to a ball also connected with the same extremity; but, when the column was active, it receded from this ball, and discharged the electricity of the positive end upon a ball connected with the negative extremity, or with the ground, or with both; after which it fell by its gravitation into the first position. He also placed a cross wire above the bead to prevent it from striking, and afterwards substituted a gilt pith ball of the size of a pea, for the bead, and extended his number of groups to 1300 of $1\frac{1}{2}$ inch square: this apparatus continued in motion more than two years, (and has not that I know yet ceased,) varying in the number of vibrations in a minute from forty-five down to scarcely one.

But Mr. B. M. Forster had constructed a similar kind of apparatus to that which M. De Luc first employed, which kept a pair of bells ringing several months; and Mr. William Allen

extended a column to 10,000 groups of small diameter, and placed them in glass tubes.

Mr. G. S. Singer has subsequently improved it materially by interposing two disks of paper instead of one, which increased its power considerably, and thus lessens the labour of construction. He extended his series to 20,000 groups, and has filled up an apparatus of two small columns in a very convenient form for ringing a pair of bells. But I have constantly preferred the second method employed by M. De Luc for observing the phænomena of this curious instrument, which I have endeavoured to render more convenient by using a much larger ball on the pendulum; by making this pendulum of an inflexible wire instead of the fine silver thread; by causing it to partake of the motion of the common pendulum with that of electric attraction, and by applying its vibrations to the motion of indexes. It would not be just to omit here my acknowledgement of obligation to Mr. Gorham, a very ingenious watch-maker at Kensington, from whom I received great assistance, and who executed the mechanism of the wheels, levers, &c. in a very neat and accurate manner.

AA, &c. (Plate V.) are six of Mr. Singer's columns in glass tubes, supported by two stems of glass covered with sealing-wax BB, and the flat pieces of brass CC, &c., which pieces serve also to render them continuous: the positive extremity P is connected by a brass wire with the dial plate D; and the negative extremity N, with the screw 6. The dial plate is supported by a stem of glass projecting from the piece of wood E behind it. The pendulum, which is a wire of steel of about 1-50th of an inch diameter and 14 inches long, carries a ball of gilt cork about one inch diameter: this when unelectrified hangs at about 4-10ths of an inch from the disk. No. 1 and 2 is a bow stretching a fine silver wire, and is attached to a spring so that it can be advanced towards or withdrawn from the pendulum by turning the screw 3. 4 is a flat piece of brass carrying the support of the disk &c. 5, and has a groove which allows the lower part of the screw 6 to pass through, so that the disk and bow may be placed and firmly secured at the required distance from the bob of the pendulum, which is ascertained by a great many trials. The whole is covered with a large glass shade. Fig. 2 is a front view of the mechanism attached to the dial plate, of the exact size of the original. No. 1 is the top of the pendulum, which is suspended from the peg 2 by a piece of fine watch pendulum wire. I have not drawn the lower part, because it would obstruct the view of the crutch 3. This crutch moves on the pivot 4, and carries at the upper end a small lever 5, which

which moves on the pivot 6, and is prevented by the screw-head 7 from moving beyond a certain point to the left, whilst a very delicate spring 8 causes it to regain that point after it has been removed from it. At the lower end of the crutch are two pins 9, which receive between them the pendulum. The wheel 10 has sixty teeth, and is supported by the cock 11. When the pendulum moves to the right, the upper end of the crutch moving in the contrary direction, and the spring 8 yielding, allows the lever 5 to clear one tooth of the wheel; but when it moves to the left, the screw-head 7 stopping the lever, the wheel is caused to move through the space of that tooth, the elbow 12 dropping between two teeth at the other part of it, and keeping it steady.

The wheel 13 has also sixty teeth, and is moved in a similar manner by the lever 14, which carries the small lever 15. 16 is a pin which acts upon the lever 14 at every sixtieth vibration; and consequently if the wheel 10 make one revolution in a minute, the wheel 13 would make one revolution in an hour. Any number of wheels might of course be set in motion, if the maintaining power on the pendulum were sufficient to overcome the friction and inertia. The indexes are fixed to the pivots on the other side of the dial plate. It will be easily understood by recurring to fig. 1, that the vibrations of the pendulum are maintained by the successive charges of electricity which the cork ball receives from the positive end of the column, and its discharges at the cross wire No. 2, where a small spark is always perceptible.

It may be also easily conceived that the rapidity of the vibrations is influenced by the variations in the electromotive power of the column, which are occasioned by the circumstances stated by M. De Luc, Mr. Singer, and myself, viz. heat, moisture, and the electricity of the ambient air. Whilst engaged in the construction of this apparatus for the purpose above stated, it occurred to me, that if the power of the column were sufficient to make the pendulum vibrate *as fast* as seconds in all temperatures, and under all other circumstances, it would be possible to draw off the superabundant electricity which at high temperatures, &c. made it vibrate *faster*, as quickly as it accumulated; and after several trials I adopted the following method, by which I succeeded better than I expected to have done, in regulating the vibrations.

No. 17, fig. 1, is a similar piece of brass to 4; it carries the support of the screw 18: this terminates in a fine point, and passes through the disk 19, which can be placed at any required distance from the point to be ascertained by experiment, and can also be advanced to or withdrawn from the cork ball *with* the

 point,

point, by turning the screw. The point is placed at a very small distance from the cork ball when the latter is in the most distant part of its vibration from the disk 1; so that, in proportion as the electricity is more abundant and intense, the disk 19 causes it to make a longer vibration, and thus to bring it nearer to the point, which discharges a portion proportionate to its proximity. The columns represented in the plate have kept the pendulum thus circumstanced in activity about three weeks. When the temperature of the room is above 53 degrees, it gains about two seconds in five minutes for every advance of one degree; but when it is below this degree it diminishes its velocity gradually, until it no longer vibrates so fast as seconds.

In this state of the apparatus, the Right Hon. Lord Henneker suggested to me a method of improving it, by connecting two or more of the columns at both extremities, which would produce a greater frequency; and I am taking advantage of this hint, by constructing a column much larger in diameter and in the number of groups.

I am, sir,

Your obliged and humble servant,

Hammersmith, March 9, 1815. FRANCIS RONALDS.

XLVI. *Remarks on Mr.* BAKEWELL'S *Geology on Northumberland and Durham. By* G. A. DE LUC, *Esq. F. R. S. &c.*

To Mr. Tilloch.

Windsor, April 5, 1815.

SIR,—I HAVE found in Art. XVII. of your Number for last January, under the title of "Observations on the Geology of Northumberland and Durham," some propositions on which I shall communicate to you my remarks, as they concern a most important point of geology; namely, *metallic veins*.

The author, Mr. Robert Bakewell, thus attacks the celebrated M. Werner (p. 91 of the Journal): "The almost invariable change in the quantity, or in the content of *metallic veins* as they pass through different rocks is, I conceive, a demonstrative proof of the fallacy of *Werner's theory*, which represents veins as *open fissures*, filled with *metallic solutions* from *above*, either by an opening at the top or through openings in the sides. Were *veins* filled in this manner, the *quality of the rock* would have little influence on the *ore*. Werner quotes an instance at *Kingsberg* in Norway, as if it were unique, of the content of the *vein* being *richer* as it passes some of the beds, than in others. It would not have suited his theory to have admitted more; but the

the fact is, that so far from this being a rare occurrence, it is almost a general law, at least in England, where I believe *mining* operations have been carried on to a *far greater extent*, and *with more capital and skill*, than in *any part of Germany*.

"The *principal substances* which *fill the veins* in this mineral district, besides *galena* or the *sulphuret of lead*, and *blend* or *sulphuret of zinc*, are *quartz*, *fluor spar*, *calcareous spar*. . . . ." The author goes on describing what is called *gangue* by mineralogists, of which the *metalliferous* part which is here *galena* and *blend*, though the object of miners, constitutes a very minute part. I might say only that the author attacks M. Werner's system without being sufficiently acquainted with it; but as this is an important point, not only in mineralogy but in geology, I shall enter into some particulars.

In the first volume of my Geological Travels in some Parts of France, Switzerland and Germany, I relate a journey which I made in the mountains of *Freyberg* with M. Werner himself. We had both happened to express in the same French journal, the opinion, that *metallic veins* were *fissures* in the *strata*, while still under the water of the sea; which *fissures* had been filled by deposits on their sides from the same liquid; a proof of which operation is, that the same deposits are found on both sides, forming what is called *gangue*, not necessarily *metalliferous*; but that some of the *gangues*, or part of a *gangue*, contained ingredients which, by smelting, produced *metals* of different sorts according to the *veins*.

The inaccuracy of the author in stating M. Werner's system, consists in ascribing to him the idea that *veins are open fissures filled with metallic solutions from above*; while he expressly considers the filling up of the veins as a *progressive* operation, by successive *deposits* from the liquid of the sea against the *sides* of the *fissures*; a proof of which operation is, that the deposits are *symmetrically* formed on both sides of the *fissure*.

I had been in that respect, some years before, in a very good school and under a very able master. The scene was the mineral country of the *Hartz*, where, contrary to the opinion of the author, the *mining operations* had been *carried on* to a *much greater extent* than *in England*, not only for the extent of the *veins*, but for the breadth and the depth in which they have been followed throughout their ramifications. In short, the *Hartz* is one of the most celebrated mineral countries in which *veins* have been worked for many centuries; and surely the author was not acquainted with it, when he thought *that in no part of Germany the mining operations had been carried so far as in England*.

My guide in that great mineral scene was my late intimate friend

friend Baron Von Reder, who was at that time captain general of the mines, in which he accompanied me himself, making me observe many particulars which I could not have discovered without him, in those dark underground passages, down to above 1400 feet. He made me in particular observe the proofs that the *veins* were *fissures* in the *strata*, produced by unequal subsidence of the sides after the *fracture*, which inequality was shown by the want of correspondence of the *same strata* on the opposite sides of the vein; making me observe some places where there was above twenty feet difference in the *level* of the same *strata* on the opposite sides of the *veins*. A clear proof that these *openings* or *fissures* in the *strata* were produced by some catastrophe, in which, after the *fissure*, one side had subsided more than the other.

With respect to the process of *filling up* these *cavities*, he gave me a proof of a *succession* in that operation, by the difference of the *metallic content* of the same *gangue:* he showed me in particular some recesses, or cavities, on the side of the *vein*, incomparably richer than the other parts, which he had caused to be shut up by doors, in order to keep them for the time when the vein was hardly rich enough to pay the wages of the miners; at which times he permitted them to extract some of that *rich ore*, to make an average of the yearly product; as these mines are the common property of companies, who are to maintain the miners at all times with the same wages, be the product of the mines more or less.

From these observations it was that I wrote a paper in the same French journal, which contained one of M. Werner's, in which he assigned the same origin to the *metallic veins*. It was therefore very interesting for me to observe with him the mineral country of *Freyberg*, in which he was also director of the mines. I wrote to him from *Dresden*, my intended visit; he was so good as to come himself to meet me at *Dresden*, and I spent some days very usefully and agreeably at *Freyberg*, making many excursions in that very interesting mineral field, which observations I have related in my Travels: but I shall confine myself here to my subject, that of the formation of the *mineral veins*, and the production of the *gangue* in those *fissures* of the *strata*. For which purpose I shall copy what I describe from p. 448 of the first volume of those Travels.

"On the side of the valley in which flows the small river *Mulda*, M. Werner made me observe several *little veins*, which having been broken in the catastrophe whence is resulted the *valley* itself, most evidently have been formed in *fissures* anterior in date to the formation of the valley. Many of these *fissures* unite below in a single one. In the part here exposed to

to view, the *gangue* was chiefly of *spar*, and contains nothing *metallic;* but when the *veins* thus collected in a *single fissure* are followed to a great depth, *ore* is found in some of them." This is a direct proof of the system which we have maintained, M. Werner and myself, for the formation of *veins;* a system very different from that which the author attributes to him, which he certainly has not found in his works.

Continuing the account of my travels, I say at p. 448, "It was in the side of this valley that M. Werner pointed out to me the principal phænomenon which had convinced him that *veins* were *fissures* filled up with substances *precipitated* against both the *sides* of the space thus opened, and that phænomenon was precisely the same which had led me also to the same opinion. In all *veins*, these new substances have on both sides been deposited in symmetrical layers; and the intervals between the sides having been gradually narrowed by their accumulation, they have at last united towards the middle; where, however, there remain some vacancies, lined with *small crystals*. Now, in the *little veins* just described, M. Werner showed me a remarkable circumstance, which at once proved the symmetrical accumulation of the substances on the opposite sides of the fissures, and the catastrophes undergone by the *veins*, after the formation of the *first gangue*. These fissures have been evidently enlarged by a new subsidence of the strata, which having been more considerable on one side than on the other, has divided the *first gangue* in many places along the line of the first junction. The same symmetrical layers uniting incompletely towards the middle, have again been formed in the same manner as before. This is a case which I have frequently observed in large *veins* where new fissures have taken place; sometimes towards the middle, sometimes on one of the sides: and where the unequal progress of the accumulation on the opposite sides is shown by effects on a greater scale, especially by large cavities, they are always lined with crystals, like that which I have described at *St. Andreasberg* in the *Hartz*, § 185." If the author, who speaks of M. Werner's system without knowing it, had only known my *Travels to Freyberg*, published in London in 1813, he would have found in it all the particulars above mentioned.

M. Werner led me to the highest point of the mineral ground of *Freyberg*, from which he pointed out to me the course of the principal *veins*, crossing each other, as it happens when the ground *splits* by dryness, or by unequal sinking.

But the mineral region of *Freyberg* has a very different aspect from what I had observed in other mineral countries. Had it not been for the *huldes* or heaps of rubbish extracted from the *mines*, and the outward machinery, it would have been impossible

to

to judge of the internal part of the ground. For the external part presented to the sight only hills with gentle inflexions, at that time covered with the finest harvest. I was struck with the idea that I walked over a land inhabited by a nation of *gnomes*, occupied down to a great depth in opening new paths in the bowels of the earth.

Lastly, I observed from another high ground, that this mineral country is confined by two valleys or great fractures in the *strata*, beyond which the *veins* no more appear externally.

I come to the author's opinion, "that if the *veins* were *fissures*, the *quality* of the *rock* could have little *influence* on the *ore*." There is no reason given by the author for his assertion, nor is the case a general one. In the *veins* of *Derbyshire*, the same *gangue* continues without interruption, and without difference in the fundamental material, through different kinds of *strata*; but it contains *galena* only when it pervades the *limestone strata*. This is a fact, but nobody that I know has undertaken to explain it. It would be incumbent on the author to prove that it is a constant effect, in order to give it as a general case, that the *nature* of the *rock* has an *influence* on that of the *veins*. But in this again he shows that he does not know the great mineral country of the *Hartz*. The whole of that chain consists of *schistus*, and there is a great variety in the content of the *veins*. Besides *galena*, furnishing by smelting *lead* containing *silver*, which is the greatest mineral product of the *Hartz*, there are *veins* of *copper* and of *iron* ores, very near one another, and in some places crossing each other.

These are facts, but till now they give no clew that leads to their explanation. The author does not increase our knowledge in that respect: on the contrary, in contradicting Werner's system concerning the *veins*, which I think I have now proved, he throws obscurity on points assented to by the greatest number of mineralogists, without substituting any thing to that system.

XLVII. *Dr.* Gregory, *in Answer to Mr.* Harvey's *Mathematical Question.*

To Mr. Tilloch.

Dear Sir,—As it will probably be expected that I should pay some attention to Mr. Harvey's question at p. 233 of your last number; allow me to say a word or two, consistently with the brevity you require.

In

In order to render the expression $\frac{\dot{\delta}}{\delta^{\frac{3}{2}} - (\delta - \alpha)^{\frac{3}{2}}}$ integrable, α being given, let both the numerator and the denominator of the fraction be multiplied by $\delta^{\frac{3}{2}} + (\delta - \alpha)^{\frac{3}{2}}$; the resulting expression will be $\frac{\delta^{\frac{3}{2}}\dot{\delta}}{3\alpha\delta^2 - 3\alpha^2\delta + \alpha^3} + \frac{(\delta-\alpha)^{\frac{3}{2}}\dot{\delta}}{3\alpha\delta^2 - 3\alpha^2\delta + \alpha^3}$. The first member will evidently be made rational by putting $\delta = x^2$, and the second by putting $\delta - \alpha = x^2$; so that both are then susceptible of integration by the well known methods for rational fractions.

Still, however, as the final expression for the time thus determined, is in some cases complex, Bossut, Prony, and others, who have treated this class of questions, recommend the approximative method of squaring a dependent curve, of which I have spoken in the article of my Mechanics*, cited by Mr. Harvey.

In reply to the optical query of your Correspondent A. M. allow me to refer him to "Harris's Optics," at pp. 120–124 of which he will find detailed some obvious experiments for the determination of "the *minimum visibile.*" They would, I fear, occupy more room than could well be devoted to them in the Philosophical Magazine, unless Harris's book should be out of print. It was published by White, Fleet Street.

I am, &c.

Royal Mil. Academy,
March 3, 1815.

OLINTHUS GREGORY.

XLVIII. *On certain Products obtained in the Distillation of Wood, with some Account of bituminous Substances, and Remarks on Coal. By* J. MACCULLOCH, *M.D. F.L.S. Chemist to the Ordnance, and Lecturer on Chemistry at the Royal Military Academy at Woolwich.*

[Continued from p. 217.]

THERE is a wide interval between the external characters of the lignites and of coal, and though we cannot presume to state the period which Nature has used in her operations, nor during how long a space the causes have continued to act, before the vegetable matter has undergone its ultimate change into coal, nor therefore whether the long continued agency of water and pressure may not have produced the required changes; yet, since

* A new edition of this work, with considerable improvements, is now in the press, and will be published about midsummer.

philosophers

philosophers of high reputation have supposed that fire has been a probable cause of this conversion, and that this theory is supported by considerable evidence in some analogous cases, it is our duty to examine by experiment, what effects conducing to this end may result from our limited trials. The foregoing experiments show that the fire of our furnaces does not convert wood into bitumen, and the processes of Nature seem to prove that water can produce this effect, and that jet, the bituminous lignite which approaches nearest to coal in its chemical characters, is the result of this action. Yet there is an interval between jet and coal, as I have already observed, requiring explanation. The chemical characters may be identical, but the mineralogical resemblance is still wanting. It is possible that the agency of fire may account for this ultimate change, and that its action on beds of lignite and peat has converted not wood but vegetable matter already bituminized by water into coal. Pursuing this train of investigations I was induced to try if jet, the most perfectly bituminized lignite, could by the application of heat under pressure be converted into coal. For this purpose I introduced powdered jet into gun barrels, placing it between two portions of rammed Stourbridge clay, with the view of absorbing a part of the distilled petroleum when it might be formed in greater quantity than was requisite for the success of the experiment, and where by its conversion into hydrogen it might endanger the bursting of the apparatus. The barrels, which were Swedish, were held in a moderate red heat till they burst, when they were instantly withdrawn and cooled in water to prevent the further volatilization of the bituminous matter. As the opening was generally no larger than a pin hole, there was no difficulty in cooling the apparatus in time. In this way, among some failures, I procured a perfect fusion of the jet, which exhibited the true characters of coal, and was taken out with the impression of the irregularities, in the barrel. I need not add that in this case the produce had not merely the colour and inflammability but the fracture of coal and its odour on burning. It is not unlikely that by a sufficient repetition of these experiments with better regulated heats and more leisure than I possessed, several varieties of coal might have been in this way produced. Indeed some of the specimens exhibited a dry, and others a fat appearance, but it was impossible in general to detach them from the barrels without reducing them to small fragments. Two other circumstances occurred deserving of notice. In one or two cases where the heat had been too great, a portion of the jet was reduced to charcoal, which continued attached to the coaly matter, and the clay was in every instance blackened to a considerable distance from the jet, and converted into a

hard

hard compact substance resembling bituminous shale in its smell and consistence.

Reverting to the chemical nature of the other lignites, there is very little reason to doubt that those among them which approach the nearest to a state of perfect bituminization, would have given results nearly similar, but I could not pursue the investigation for want of sufficient specimens. From peat we should expect but a mixed matter, varying between the bitumen of wood and true bitumen, according to the degree of change previously undergone; for that the process of bituminization is the effect of water, and not of fire, is rendered probable, as much by these trials as by the geological observations above mentioned. The conversion of bituminized wood into true coal may possibly be the effect of a consolidation produced by the agency of fire; but I shall leave this argument in the hands of those who have undertaken the defence of this theory—having entered into this train of reasoning, not by design, but from the unavoidable concatenation of experiments *.

A circumstance occurred in the coaly residuum of the wood tar which it is worth while to notice, although of an accidental nature, and not essentially affecting the history of the vegetable bitumen or pitch which I have described. It bore no resemblance to common charcoal, but was more like black lead. It was as glossy, and although not so soft, marked paper with a similar streak. It was inflated, and therefore minutely scaly, and porous, and was attracted by the magnet. Muriatic acid took up a portion of iron from it, as it does from many varieties of plumbago, and the remainder resembled plumbago after it had been submitted to the action of acids.

It was also exceedingly difficult to burn, requiring a long con-

* That I may not interrupt the text, I will add, in a note, a cursory account of the black matter which is deposited in bogs, and which seems to be the substance giving the pitchy appearance to the more compact varieties of peat. I have not seen it in the soft state in which it is first procured.

When dry, it is black, sometimes dull, sometimes with the lustre of asphaltum. It is heavier than water. It is not electric. It is brittle, and breaks with a fracture intermediate between the splintery and conchoidal, resembling asphaltum generally in its external characters. Exposed to a red heat it is incinerated, giving a smoke possessing a modified smell of vegetable (pyroligneous) acid. It is not acted upon by boiling alcohol, ether, or naphtha; and in this latter circumstance its difference from asphaltum is marked. Neither is it soluble in boiling water. It is readily dissolved in lixivium of potash, and by nitrous acid. It appears to be formed of the vegetable elements in the state of transition to bitumen, the carbon having been first held in solution, as it is in the water of dunghills, by the other matters with which it was combined, and being at length consolidated by the dissipation of a portion of them. The produce of its combustion shows it is combined with both hydrogen and oxygen.

It

tinued red heat, after which it left an oxide or rather a carbonat of iron, such as remains from the combustion of plumbago. It is in fact to be considered as an artificial plumbago, a substance of whose nature all the charcoals of difficult combustibility partake, deriving their resemblance apparently from the same cause.

The formation of this plumbaginous substance serves to show a very powerful affinity between iron and carbon, even where the proportions are very different from those which enter into the composition of steel. But to effect this combination, it is necessary that the carbon be in a state of previous union with other substances, and that it be applied to the iron in that state. It will be in vain that we attempt to combine iron with charcoal for this end, unless the charcoal or carbon be in that state of very minute division in which it exists when precipitated by a new affinity from some previous combination.

It is necessary now to account for the iron in this compound. This distillation of wood for charcoal is carried on in iron vessels, and hence is derived the iron which enters into the composition of the pitch. I will not say that it is solely derived thence, as it is probable that if there were iron contained in the vegetable matter, it would also be found in the same place. When the acetic acid has been separated the iron remains united to the pitch. This fact may show us, that if in the destructive analysis of vegetable (and probably animal) matter, we trust to find the iron they may contain in the residual matter of the distillation, we may be disappointed, since it may be carried over, together with the substances I have now been describing, in the act of ebullition, as happens in this very case, its tendency being to combine with them, in preference to the charcoal.

As it was no part of my design to examine the vegetable elements, I did not pursue any experiments with this substance distilled in earthen vessels so to ascertain whether in this case also it would contain iron, but I did enough to satisfy myself that the pitch was essentially the same in whichever way produced.

It is already known that a substance resembling plumbago is formed in water, it having been discovered by Fabroni in the country round Naples. It is equally known to be formed in the iron foundries; and the advocates for the igneous origin of coal have also contended for that of plumbago, and have supposed it to have been produced by the contact of melted greenstone with beds of coal. But even if we admit this cause of its formation, something else seems necessary for the production of the substance, and some other mode of applying the heat required before it can be produced. Nor indeed does the explanation sufficiently correspond with the general geological position of plumbago.

In

In numerous trials to combine iron with charcoal so as to form this substance, I have uniformly failed of success, except where, as in the case above related, the charcoal or carbon has been in a state of previous combination, or was actually held in solution. In many trials on this principle, the results have been tolerably successful. If therefore we are to adopt an igneous theory of the formation of plumbago, it will be as easy to suppose that the action of subterraneous fire on mixtures of bitumen and iron has produced the compound of charcoal and iron, on the principles I have described, and this supposition will be more consonant to the chemical facts. But we are too little acquainted with the geological relations of plumbago to lay much stress at present upon this or any other hypothesis. It is evident that plumbago may be a produce of art; and could it be produced in as solid and compact a state as Nature affords it, the discovery would form a material addition to those useful ones for which the arts have been indebted to chemistry.

As nothing tends more to confusion of ideas than confusion of terms, I may be excused for proposing a name to the pitch of distilled wood, a name in familiar use, though hitherto unappropriated by chemists. It is in fact that which is well known to painters by the name of *bistre*, although the nature of bistre has I believe never yet been examined; and the importance of it to the arts of design induces me to extend this article for a few lines. According to Dr. Lewis, bistre is produced from the soot of all wood, other receipt books give us the same account, but limit the sort of wood to beech without seeming aware of its real nature; but the colourmen use the soot of all wood indiscriminately.

Those artists who have made the tour of the highlands of Scotland, are well acquainted with that variety of it which varnishes the interior of a highland cottage.

In all these cases it is a very variable article; and the colourmaker, being unacquainted with its real nature, is unable to rectify its faults; in consequence of which it is often unfit for use, notwithstanding the various operose and mysterious purifications it undergoes in his workshop. The causes of these varieties will be very evident to those who have read the foregoing experiments. An imperfect separation of essential oil and a consequent tenacity arising from its too near alliance to the tar, will appear to be its most common vice, and it is this which gives it that disagreeable gumminess and disposition to return to the pencil which is destructive of its best qualities. At times also from the same causes it is offensively yellow. So valuable is a brown colour that will work freely and with transparency, that the artists will be much obliged to him who shall render bistre

 equal

equal in freedom and force to seppia. By distilling or evaporating the oil from the pitch, according to the process described above, a colour may be produced varying in tone from the warmest bistre brown down to black. At the same time the substance loses a great portion or the whole of its disagreeable tenacity, according to the degree of boiling it has undergone. By treatment in alcohol, results in some measure similar are produced, and the residuum of this solution is equal in colour to seppia, and totally void of tenacity. In either or both of these ways may the quality of this colour be improved.

It might perhaps be a matter worthy of trial, whether useful varieties in colour and quality might not be produced by the distillation of different woods. That which I used was procured either from willow or alder—the two woods chiefly used in the royal powder-mills, but I cannot ascertain from which of them. The solution in lixivium of potash or of soda, a substance analogous to the resinous soaps, answers the purpose of ink, possessing a colour sufficiently intense and flowing freely from the pen without requiring gum. As it is indestructible by time, by the common acids or by the alkalies, perhaps it may be found a valuable substitute for this useful but fugacious substance. The compound of bistre and soda appears peculiarly well fitted for drawing in monochrome, since, as it does not consist of a powder suspended in a vehicle, it is free from the peculiar defects, so well known to artists, which occur in colours thus compounded.

I may also add that it forms a substitute for asphaltum in drying oil, where such a coloured varnish is wanted, and that it makes a very good japan varnish for metal if dissolved in spirit of wine, and heated strongly after its application. It is for practical men to see whether by combining it with asphaltum, lac, or the gums, some more useful and cheap compounds of this sort may not be produced.

XLIX. *An earnest Recommendation to curious Ladies and Gentlemen residing or visiting in the Country, to examine the Quarries, Cliffs, steep Banks, &c. and collect and preserve Fossil Shells, as highly curious Objects in Conchology, and, as most important Aids in identifying Strata in distant Places; on which Knowledge the Progress of Geology in a principal degree, if not entirely, depends. By a* CORRESPONDENT.

To Mr. Tilloch.

SIR,—HAVING long been a constant Reader of your very useful Magazine, I have been induced by what I have therein read regarding Geology, to become a reader and very zealous promoter of

of Mr. *James Sowerby's* work connected with this interesting subject, entitled "Mineral Conchology," published every two months: and in order that I may have the opportunity of recommending many more Ladies and Gentlemen in every part of Britain to become *Collectors of Fossil Shells*, I have been at the pains to collect out, alphabetically, from the sixteen numbers of Mr. Sowerby's work already published, and send you herein, the *Names* and number of *presents* or *loans* of Shells, which have been respectively made by them, for enabling Mr. Sowerby, in addition to his own extensive Collection, to proceed thus far in his meritorious design, of publishing coloured Engravings and Descriptions of all known Fossil Shells, and their habitats, those of England more particularly.

The Names of these public-spirited Individuals, recorded as *the collectors and contributors* of Specimens of Fossil Shells, which are already described or mentioned in Mr. Sowerby's work, are as follows, viz.

Rev. Dr. Charles Abbot ..	1
Mr. Arthur Aikin	3
The Countess Dowager of Aylesford	3
T. J. L. Baker, Esq. ..	4
The Marchioness of Bath ..	1
Miss E. Bennet	10
Mr. Benjamin Bevan ..	1
Rev. W. Bingley	5
William Borrer, Esq. ..	3
Mr. Boys	1
James Brodie, Esq.	1
The late Mr. Bryer	1
Mr. William Bullock	1
Anthony Carlisle, Esq. ..	1
Dr. Clarke	1
Mrs. Cobbold	11
Mrs. Codrington	1
The Right Hon. Lord Compton	1
The late Mr. William Cunnington	5
The late General Davies ..	1
Mr. Downs	1
Mr. John Ducket	3
Richard Duppa, Esq. ..	1
Mr. John Farey sen.	2
Rev. Mr. Fearon	1
Rev. Dr. John Flemming ..	5
Mrs. Gent	2
Mr. James Gibbs	11
George Bellas Greenough, Esq.	2
Rev. John Hailstone ..	1
Mr. Samuel Hailstone ..	1
Colonel Hardwicke	1
Rev. Mr. Harriman	1
Miss E. Hill	11
Mr. John Holloway	3
Mr. William Hooker	2
Sir Edward Hulse, Bart. ..	1
Mr. J. Humphreys	1
Rev. F. Iremonger . ..	2
Mr. Thomas Andrew Knight	1
A. B. Lambert, Esq. . ..	1
Rev. J. Lambert	1
Rev. P. Lathbury	2
Dr. W. E. Leach	2
Rev. G. R. Leathes	1
Charles Lyell, Esq.	1
Alexander MacLeay, Esq.	1
G. A. Mantell, Esq. . ..	12
Rev. Thomas Olbers Marsh	5
The late Mr. William Martin	5
Thomas Meade, Esq. ..	7

Mr. Milne	1	Mr. Richard Taylor, jun. ..	1
Mr. T. W. Moore	4	Mr. Thomas	1
Mrs. Morris	1	Mr. John Thurtell	1
Mr. James Parkinson ..	3	The late Mr. Trimmer ..	1
Rev. Thomas Rackett ..	2	Mr. Dawson Turner	2
John Rogers, Esq.	1	Mrs. Tylee	4
Mr. James Ryan	2	Mr. W. Walter	1
Mr. Salmon	2	Mr. Weatherell	1
Mr. Jonathan Salt	2	Mr. Henry Warburton ..	1
Mr. Sheffield	1	Mr. R. Weeks	1
Mr. William Smith	1	Lady Wilson	1
Mr. G. B. Snow	2	Mr. Winsor	1
Robert Sparrow, Esq. ..	2	Dr. Wood	5
Rev. Henry Steinhawer ..	10	Samuel Wright, Esq. ..	7
Rev. Dr. Sutton	3		

I indulge the hope, sir, that the above respectable List will enable me, with some effect to plead the cause of Geological Science, in inducing country Gentlemen of leisure, and ingenious Ladies in particular, to imitate the praise-worthy conduct of those mentioned, and turn their attention to the very interesting facts around them; viz. of there being *imbedded in almost every thick stratum* or rock, those of Limestone in particular, an almost infinite number of individual *Shells, belonging to a vast number of new species, and even new genera*, which have never yet been noticed by writers, long and ardent as has been the research of Naturalists, since the days of the illustrious Linnæus, in describing every *recent* species of Shells, throughout the known world.

Besides this important object, of increasing our knowledge of the vast number and variety of the organized and once living Beings, with which the all bountiful Creator has seen fit to people this terraqueous Globe; the entombment of an extensive series of *species*, most of them in vast numbers, each *in distinct strata*, lieing upon each other in a determinate order, through considerable tracts of country, and perhaps almost throughout the earth's surface, irresistibly leads us to the fact, of *the very slow and progressive formation of the solid matter of the Earth*, and enlarges our comprehensions of its great Author, by making us in a considerable degree acquainted with, events long prior to, and which yet are in perfect unison with the sacred writings, which commence not with *details* of the creation of the Earth itself, much less of its early and progressively entombed animal Inhabitants, but with what more immediately concerned our early fore Fathers to know, viz. the origin of *the existing species* of Plants, Animals and Men, *upon the surface* of the earthy ball, and in the waters, now covering only *a part* of this surface, instead of such being previously,

previously, *the universal cover* of the solid land, as the Mosaic records attest, and as innumerable *facts still observable* within the earth's surface, concur in proving to have been the case.

I was long myself, after I had begun to take views of the subject, not very different from the above, under the then too common mistake of supposing, that, in order to be able to contribute in any useful degree towards the stock of Geological knowledge, which even yet is lamentably deficient, it was necessary to possess a considerable and *technical* knowledge of *Mineralogy*, (including its chemical and crystallographical departments,) of Botany and comparative Anatomy, of *Conchology* in particular, with ample leisure and opportunities for employing these, *in foreign countries*, more than in our own, which last we had been often told, was barren to a proverb, of true Geological phænomena ! !

A sight however with which I was favoured, of Mr. *William Smith's* Collection of Fossil Shells and other Organic Remains, arranged in the order in which they are found *in the several Strata* described by colours on his Map of England, and each Specimen distinctly marked with *the name of the Place* from whence it was taken, at once convinced me, that what had been achieved, by the perseverance of a plain and moderately lettered Man, in a great measure if not totally unacquainted with the *technical* knowledge above mentioned, might be followed by others; and I became convinced, that the efforts of persons like myself, and numbers of my acquaintance, residing always or frequently in the Country, might contribute usefully to Geology: and since Mr. *Sowerby* has begun the Work, which it is now my object to recommend, the same has been evident to most of the Individuals in the above List, and to his Readers in general, and will I trust prove so to daily increasing numbers, everywhere.

Industry and attention only are wanted for Ladies or Gentlemen to be able, in most country situations, to collect many fossil Shells, either from Quarries or Pits which are constantly in the course of work, by small occasional gratuities to the Workmen for preserving all they meet with, and *pointing out the exact bed of stone, clay, &c. in which they each lie*, entombed; by the like attention to occasional opportunities of seeing the strata penetrated by new Wells, Shafts, Drains, Foundations, &c. or after the slipping down of Cliffs or steep banks after heavy Rains or Frosts, &c. in order to collect the shells *newly exposed* thereby, and record their exact matrix or place in the strata, (by their local appellations as well as by their ordinary names, as Limestone, Chalk, Sand-stone, Sand, Clay, Marl, &c.) which are even still more important, because resident Observers can alone collect these occasional facts, while the regular Quarries and Pits

may be, and many of them already have been visited, by travellers in search of Geological facts, like Messrs. Smith, Farey, Bakewell, Greenough, and many others; this latter circumstance ought not however to occasion the neglect of such public and permanent Works in the strata, because the deeper and further progress of excavating, is continually exposing new or better specimens of Shells than have hitherto been collected, in almost any Quarry or Pit.

Each Specimen should have *a number* put on it, as soon as possible after collecting, referring to the *Notes* taken, as to its habitat or place in the strata, as above mentioned, and the Place on the surface of the country, described by the *Name of the particular quarry* or work, and in what Farm, Township or Parish situated, and *its bearing and distance*, as near as these can be estimated or told by the workmen, from the nearest church or village which is shown in the ordinary Maps, not forgetting to add the County to all such descriptions, for fear of another place of the same name being confounded therewith, by others to whom these particulars may be communicated.

A common mistake by many Collectors has been, imagining that they were possessed of *unique* specimens of fossil shells, and which persuasion has had two bad effects, one of them occasioning the neglect of an accurate and persevering search in *the identical stratum* from whence their rare specimen was taken, by which other such specimens might almost with positive certainty be found; and the other evil resulting, has been, that so high a value was set on the supposed unique specimen, that such Gentlemen as Mr. Sowerby have rarely been able to obtain even the loan thereof: whereas, the profusion that exists of fossil specimens, *in their proper beds*, might enable all industrious Collectors to have so many almost equally perfect duplicates of each species in their Collections, as freely to give or exchange with all their curious Friends who may visit them, and even to enable them, occasionally, to carefully pack up a small Box of good Specimens, as to their being pretty entire, and showing the *hinge* of the Shell in particular, each numbered, and accompanied by a numbered extract from the collector's Notes (as above mentioned), and forward the same by some careful Friend, or what is better, by the stage Coach, directed for Mr. James Sowerby, No. 2, Mead Place, near the Asylum, Lambeth. I have not experienced or understood, that Mr. S. in any case objects to paying the carriage of such presents; but it will I think occur to most of such liberal contributors, towards a work of general interest, that *he* should not be so burthened, when great numbers of *duplicates of the species already in his collection* may flow in upon him, and where, yet, it

it is most desirable for the interests of Science, that they should so accumulate, as the only means of enabling him to deduce the true characters and *nice distinctions which are necessary* in describing Fossil Shells, so as to render the same useful in Geological investigation.

It remains for me to mention one other very important caution to Collectors of Fossil Shells and mineral matters, which is rendered necessary, by the almost universal scatter of *alluvial rubbish* on the tops of the regular and undisturbed Strata, such alluvium being often accumulated to a vast thickness, of alluvial Clay in particular, and in which, as well as in alluvial Sand, and the more obviously worn Gravels, fossil shells are commonly found, mostly in a worn state, but not invariably so. The careful Observer need in no instance to be in doubt, as to distinguishing *alluvium* from *strata*, if he will attend to the regularly laminated and unmixed nature of the latter, except as to *grains* they may be composed of (as Sand, Sandstone, &c.) or *nodular masses*, (like those of Flint, Ironstone, &c.) and *organized remains* they may contain, which are regularly disposed in strata, in most instances; while on the contrary, the alluvial matters, always found upon and *never under any regular stratum*, are disorderly mixtures, containing *water-worn and violently broken pebbles* and stones, most of them entirely foreign to the district in which they now lie, and which may, some of them, have come from the antipodes, for aught that has yet appeared to the contrary: there is likewise an intermediate class of loosened substances found, either under the gravels and other alluvia, or nearly naked on the surface, which is called *Rubble*, Ramel, Stone-brach, &c. in different districts, consisting for the most part of angular and unworn fragments of the stoney stratum or Rock immediately beneath, *merely loosened up*, sometimes without the intervention of any extraneous matters between these small loosened stones, but more commonly, some of the loosened matter of the next superior stratum of the series, and more rarely, gravel-stones, alluvial-shells, &c. have fallen in among these Rubbles:—on all these accounts, the caution I have mentioned is abundantly necessary, always *to note it* on the spot, where fossil shells *were found in the alluvium*, and to call these *alluvial shells*:—of which latter class, any particular specimen may *appear to be unique* in the district where it is found, which in reality is no more so, than the finding of a single Oyster, Cockle or Muscle Shell, in ploughing a Field, carried thither in the manure, after having been brought from the Sea by Man, would entitle this shell to such a character of rarity.

Hoping that I shall ere long see an increase of Communications, from Men *practically* concerned and versant in the mineral

concerns of Britain, in the Geological department of your very useful work,

I remain, sir,
Your obedient servant,
April 4, 1815. A CONSTANT READER.

L. *Explanation of certain Improvements in the Construction and Fastening of the Frame Timbers of Ships or Vessels, either when Building, or when under Repair, for the more effectually preventing the Disunion of the Parts caused by Hogging, and the transverse Bending of the Hull. By Mr.* JOHN WALTERS, *Architect, Fenchurch Buildings, London.*

BEFORE I proceed to explain the proposed improvements, it is necessary to give an idea of the common method of ship-building, and of the principal improvements which the art has hitherto received.

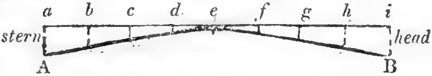
The frame or skeleton of a ship, built according to the common mode of construction, is composed of a number of curved timbers (called *ribs*) placed at certain distances from each other, in planes, at right angles to the keel on which they all rest. In the intermediate spaces between the said ribs, are placed filling timbers, answering to the curve of the ribs, so that the void between timber and timber is from one to five inches. Athwart this frame are laid beams, at right angles to the keel, serving to hold together the sides of the ship and to support the decks. And for the purpose of strengthening the parts, a second range of curved timbers or inside ribs (called *riders*) are placed over the interior planking, but at a greater distance from each other than the ribs properly so called.

As in this construction all the timbers are disposed at right angles to the keel and keelson, and the planking both within and without is longitudinal, there is evidently no mechanical support to resist the action of gravity, from which radical defect all ships have a tendency to arch longitudinally and bend transversely by the sinking of the sides. The remedy of these evils is a great desideratum in naval architecture.

The constant strain which a ship experiences from an unequal distribution of weight, and the upward pressure of the water, is the cause of its becoming arched throughout its length, or (as it is called) *hogged*, to which cause may be added the partial pressure of the water in a longitudinal direction, which tends to compress the keel, and thereby to increase the curvature. The degree of curvature produced by these combined causes is

is of course different in different ships. Dr. Young has investigated this subject, the detail of which will be found in a very interesting paper published in the Philosophical Transactions, Part II. 1814. The result of his calculations applying to a 74 gun ship is as follows:

"Let AB represent the line of keel of a vessel which



has hogged—imagine the line *a i* to be parallel to the horizon, touching the curve in *e*, and let it be divided into eight equal parts as in the figure. Then if we suppose the ship to be constructed of an uniformly flexible substance, the curvature will be such, that the fall of the keel below the imaginary line at each of the points *a*, *b*, *c*, &c. is proportional to the following numbers, viz. at *a*, 17153; at *b*, 09421; at *c*, 03738; at *d*, 00809; at *e*, 00000; at *f*, 00854; at *g*, 03721; at *h*, 08041; and at *i*, 13525."

The two causes above mentioned, by which the arching of the ship is produced, act permanently; but when a ship is riding over the waves, she is occasionally borne up by the fluid in her central part only, while the head and stern are forsaken. The strain hence produced, together with the agitation by which it is accompanied, far exceeds that arising from the mere inequality of the distribution of weight and pressure. Dr. Young estimates it, in certain cases, at nearly three times as much; and notwithstanding the temporary action of this cause, he justly considers it as the most destructive, because, when the fastenings have been strained and loosened by the action of the waves, the ship will more readily give way to the operation of the permanent causes.

A degree of curvature is also produced in a transverse direction, that is, at right angles to the foregoing, by the weight sustained by the sides being heavier than that in the middle, while the vertical pressure of the water is greatest in the neighbourhood of the keel. Thence arises an alteration in the transverse section of the ship, which assumes a form approximating to that of the annexed figure.

And lastly, there is also a tendency to a lateral curvature. When a ship is "hove down" on her side, there is an obvious strain tending to produce this effect; and when a series of large waves strikes a ship obliquely, they must often act in a similar manner with immense force.

The principal improvement which has hitherto been made with

with a view to counteract these evils inherent in every ship, and by which the lives of so many brave men have been lost, consists in employing oblique timbers. Mr. Seppings, who is the author of this improvement, fills the spaces between the frame timbers nearly to the height of the orlop or lower tier of beams, so as to make this part of the fabric solid; and omitting the inside plank, he places immediately over the frame timbers oblique riders, at an angle of about 45 degrees, (instead of placing them at right angles with the keel, as in the common construction,) and so disposed that the direction in the fore is contrary to that in the after part of the ship, and their distance asunder from six to seven feet or more; the upper ends abutting against the shelf-pieces (answering to the clamps of the common construction) which support the beams of the deck and the lower ends against the limber strakes. He next places two series of timbers longitudinally upon the joints of the frame timbers at the floor and first futtock heads, their ends dowelled to the sides of the riders. The framing thus formed by the oblique riders and longitudinal pieces represents two tier of compartments from the head of the floor timbers to the underside of the orlop deck, each compartment being of the figure of a rhomboid, in which is introduced a diagonal timber with an inclination opposite to that of the riders, thereby dividing it into two parts. Mr. Seppings also substitutes for the internal planking of the sides oblique braces placed in ships of war between the ports, the raking direction of these braces being lowest towards midships, and in the space on each side the centre port are placed two braces crossing each other. These comprise the principal improvements made by Mr. Seppings. Some others have also been introduced by him, but not necessary to be here noticed.

Dr. Young has published in the Philosophical Transactions his remarks upon this construction. He allows the use of oblique timbers to be good in principle. His arguments sufficiently prove their general utility, in tending to resist a change of figure in the ship; but he seems to doubt of the complete efficacy of the oblique riders and framing in the hold, when employed for the thick stuff of the ceiling. The filling in, however, appears to him wholly unexceptionable, and the braces between the ports to be decidedly more beneficial than the planks for which they are substituted.

Excepting Mr. Seppings's improvements, I know of none of any importance that have been introduced into naval architecture for ages; but valuable as they are, there was room left for further improvement, and a remedy for hogging still remained a desideratum. Having, as I conceive, contrived means for effectually obviating this defect in all ships, and having obtained His Majesty's

jesty's Letters Patent for my invention, I am induced to submit the following explanation of it to the consideration of the public, and of those who may be interested in such a subject.

The mechanical principle upon which this improvement is founded is that of forming a complete and integral truss or support from the centre of gravity (whence the strength of the whole structure should as much as possible be derived), and so connecting the parts together, by embracing the entire fabric, that any tendency to a change of figure may be powerfully counteracted, and as much strength be given as the nature of materials will permit.

The frame or skeleton of a ship being constructed in the common way, a principal frame A (fig. 1 and 2, Pl. VI), constructed of metal, or of wood and metal, is introduced in the space between the ribs, intersecting the vessel athwart the centre of gravity, connected across the ship by a tie-bar or beam B (fig. 2), and having the internal diagonal braces C, C. Also from the upper part of the said principal frame, the braces DD (fig. 1), constructed of metal, are let in flush upon the outside and bolted to the frame timbers, and carried down in an inclined direction (being that of the shortest distance over the curved surface*) to the lower part of each extremity of the vessel, and connected each with its opposite one, in the concave parts of the bottom, by bolting through from one to the other, or by any other convenient method. Thus the whole frame of the ship is firmly connected together, and the weight of either end (supposing the ship supported under the centre of gravity) is counterbalanced by the other end, while the vertical strain, proceeding from the weight of the whole fabric, is transferred by the tension upon the braces C, C, to the principal frame A. And also for the more perfect stiffening of the hull (when the spaces between the timbers are not filled in solid), and thereby obtaining a counter resistance to the weight of the middle (supposing the vessel supported at the ends); chocks or strutting pieces of timber scantling are tailed in between frame and frame behind or within the braces, which acting as arches when confined by the braces, discharge the weight of the vessel upon the butments at each extremity, which completes the truss. And it will appear evident, that so long as the truss remains perfect, not any weight or pressure can alter the relative position of any of the parts, whether the vessel is horizontal or inclined, unless caused by

* The utility of keeping the braces in the direction of the line of *shortest distance* is, that it presents the direct tension of the braces to every endeavour of the ship to change her figure, and prevents the possibility of the braces being moved by any force into a new position.

some

some shock that would go near to effect the total destruction of the whole fabric.

The foregoing description being confined to the method of trussing smaller ships or vessels, the manner of extending this system (which with a little modification is applicable to ships of the greatest magnitude) is shown by figure 3, in which A is the principal frame; and B, B, the braces, as already described; CC are auxiliary braces; D, D, minor principal frames, one in the fore and the other in the after part of the body; EE are minor braces; and F is an horizontal brace, connecting the heads of the minor braces, and forming a kind of longitudinal hoop to the upper part of the structure*. Chocks or strutting pieces are also tailed in at the back of these braces, similar to those before described, when the spaces are not filled in solid.

In ships of war the disposition of the principal braces ought to be so regulated as to be but little exposed to be shot away, and auxiliaries should be employed below low-water line, for additional security.

The braces before described are made of flat bars of metal in pieces of convenient length, so adjusted that the parts at which they are joined together, fall in the spaces between the ribs of the vessel, being connected by scarf joints wedged up for the purpose of equalizing the tension. The outer surface of the braces is kept flush with the frame timbers, and the bolt-holes in all of them are countersunk to receive the heads of the bolts, so that no part may present any obstruction to the planking.

In addition to the security afforded by this principle of trussing in constructing the ribs, I affix upon the sides over each butting joint of the timbers of which the rib is composed, a plate of iron or other metal extending above and below it, and bolted through the rib, by which means the ribs acquire great additional strength; an object of the first importance, even if only partially employed in assisting the weaker parts, and protecting those most exposed to injury; as, in cases of a ship's grounding, it commonly happens that the floor timbers are forced in at one end, and the first futtocks broke off, while, by the tendency of the sides to sink, a transverse curvature is occasioned by a failure of the parts situated near the head of the floor timbers. In many other respects the employment of these iron clamps will be found of the most essential service.

Such is the general arrangement and application of my inven-

* As the tension upon the horizontal brace F is in the direction of the planking, this brace is not necessary to be used except in cases where great additional strength is required.

tion

tion to practice. It is a system of framing, which resembles in some of its qualities, and in its effects and operations, the trussing employed in the construction of roofs, bridges, and other framed carpentry of great span, so that a vessel to which it is applied may not improperly be called a trussed ship or vessel; and this powerful support, superadded to the usual fastenings, must, it is presumed, infallibly produce the great objects proposed of preventing the depression of the two ends, and the butt ends and seams of the plank from opening, and also give general strength and security to the vessel.

The advantages resulting from this improvement are:

First,—That the durability of vessels will be increased, thus precluding the early and frequent repairs rendered necessary by the radical weakness of the present mode of construction.

Secondly,—That by means of the powerful support effected by the truss the filling timbers between each rib may be omitted, thereby making a very considerable saving in the first cost, after allowing for the expense of the truss.

Thirdly,—The great benefits eventually to accrue to the mercantile world are, first, a reduction in the charges of freight, proportioned to the diminution of the expense of building, the less frequency of repairs, and the comparative greater durability of the vessel; secondly, a more perfect security of the cargo from damage; and thirdly, a lower rate of insurance.

Fourthly,—The saving annually to their relatives, and to the community, a great number of valuable lives.

LI. *On the Origin of the Pyramids of Egypt, and the Intention for which they were erected.* By EDWARD DANIEL CLARKE, *LL.D.*

[Continued from p. 200.]

AFTER the numerous accounts which, during so many ages, have been written to illustrate the origin of the Pyramids, it is not probable that any new remarks will meet with much attention. Yet how few, among all the authors who have undertaken to investigate this subject, have ever ventured to express an opinion of their own! Struck by the magnitude of the objects themselves; by their immense antiquity; and by a consciousness of the obscurity in which their history has been veiled, every succeeding traveller contents himself with a detail of the observations of his predecessors, only showing the extent of the labyrinth wherein he is bewildered. Yet something perhaps might

be

be accomplished, were it allowable, upon good authority, to annihilate a most redundant source of error and imposture. With this view, it may be advisable to abandon all that Grecian historians have written upon the subject. The arrogance and vanity with which they endeavoured to explain every thing consistently with their own fables and prejudices, caused the well known observation, made by an Egyptian priest, who, according to Plato, maintained that the *Greeks were always children, and had no knowledge of antiquity*. Hence originate those difficulties mentioned by Pauw, as encountered by persons who study the monuments of a country concerning which the moderns have conspired with the ancients to give us false ideas. "The latter indeed," says he (Philosoph. Diss. on the Egyptians and Chinese, vol. ii. p. 43. Lond. 1795,) "were probably deceived by being at the discretion of a set of men called interpreters, whose college was established in the reign of Psammetichus, and who might be compared to those people called *Ciceroni* at Rome. Travellers who went and returned, like Herodotus, without knowing a word of the language of the country, could learn nothing but from these interpreters. These men, perceiving the inclination of the Greeks for the marvellous, amused them, like children, with stories inconsistent with common sense, and unworthy of the majesty of history." If we would obtain authentic information concerning the earliest history of the Egyptians, we must be contented to glean from other sources; and principally from Jewish and Arabian writers. The Jews, by long residence of their forefathers in Egypt, and also by the constant intercourse offered in the contiguity of this country and India, were of all people the most likely to have preserved some knowledge of Egyptian antiquities: and the Arabs have preserved not only the names bestowed upon the Pyramids from the earliest times, but also some traditions as to the use for which they were intended. By the dim light thus afforded, and by comparing the existing remains with similar works in other countries, and with the knowledge we possess of the customs of all nations in their infancy, we may possibly attain something beyond conjecture, as to the people by whom the Pyramids were erected, and the purpose for which they were intended. The epocha of their origin was unknown when the first Greek philosophers travelled into Egypt. They are even more ancient than the age of the earliest writers whose works have been transmitted to us. That we may arrive, therefore, at any thing like satisfactory information concerning them, the following order of inquiry may be deemed requisite:

I. Who were the *inhabitants* of this part of Egypt in the remote period to which these monuments refer?

II. Is

II. Is there any thing in the Pyramids, as they now appear, which corresponds with any of the known customs of *this people?*

III. Did any thing occur in the history of the *same people* which can *possibly* be adduced to explain the present violated state of the principal pyramid?

IV. Doth any record or tradition attribute the origin of the Pyramids to *this people*, or to a period equally remote with that of their residence in Egypt?

If the *last three* of these queries admit of an answer in the affirmative, and a satisfactory reply can be given to the *first*, the result will surely be, either that we do possess documents sufficient to illustrate this very difficult subject, or, at least, that a very high degree of probability attaches to the opinion thereby suggested; and that the obscurity in which this part of ancient history has been involved, is principally owing to the cause assigned by Pauw, namely, to a train of theories founded upon the bewildering fables of the Greeks.

To proceed, therefore, according to the proposed method of investigation—

I.

Who were the inhabitants of this part of Egypt in the remote period to which these monuments refer?

The kingdom of Egypt, according to the best authorities admitted in chronology (see the calculation of Constantine Manasses), had lasted about 1700 years at the conquest of Cambyses (B. C. 525). The *first princes* spoken of in sacred Scripture are those "of Pharaoh," mentioned in the books of Moses (Gen. xii. 15), near 2000 years before the Christian æra. The *first pyramid*, according to Herodotus (Euterpe, c. 101) was built by *Mœris*, the last of a line of kings from *Menes* to *Sesostris;* and therefore it must have been erected some ages before the Trojan war. Without, however, placing any reliance upon this record, or attempting to assign a particular epocha for any one of these monuments, we may venture to assume, as a fact, upon the authority of all writers by whom they are noticed, that they existed above 1600 years before the birth of Christ. Almost a century before that time, the prosperity of Joseph, then a ruler in this country, and a dweller in the very city to which these monuments belonged, is described as having extended "*unto the utmost bounds of the everlasting hills.*" These words (Gen. xlix. 26), as applied to the place of his residence, and the seat of his posterity, are very remarkable. He "*bought all the land of Egypt for Pharaoh,*" reducing all its independent provinces into one monarchy. The entire administration of this empire

empire was intrusted to him; for Pharaoh said, "*only in the throne will I be greater than thou.*" (Gen. xli. 40.) In the remote period, therefore, to which the Pyramids refer, "*Joseph dwelt in Egpt, he and his father's house.*" It is said of them (Exod. i. 7) that they "*increased abundantly, and multiplied, and waxed exceeding mighty, and the land was filled with them.*" The customs of embalming bodies, and of placing them in sepulchral chambers, were then practised; for Jacob was embalmed (Gen. l. 2), and "*gathered unto his fathers in the cave of the field of Ephron.*" At the death of Joseph, he too was embalmed (Gen. l. 26), but not "*gathered unto his fathers.*" He was entombed, to use the literal expression of the LXX, *ΕΝ ΤΗΙ ΣΟΡΩΙ*, in Egypt. And this mode of his interment suggests a reply to the second question before proposed.

II.

Is there any thing in the Pyramids, as they now appear, which corresponds with any of the known customs of this people?

The nature of a *Soros* has been repeatedly explained, upon the indisputable authority of inscriptions where this name has been assigned to a particular kind of a receptacle for the dead, one of which now exists in the chamber of the principal pyramid. This kind of coffin has sometimes one of its extremities rounded, and sometimes both are squared; but its dimensions are almost always the same, and it is very generally monolithal, or of one stone. This is the kind of coffin which the Romans call *sarcophagus*, and any doubt as to its use seems to be without reason; because the *Soros*, in many instances, has borne not only its name inscribed upon it in legible characters, but also the purport for which it was intended. The principal pyramid, therefore, contains that which corresponds with the known customs of a people who inhabited Egypt in the remote period to which the Pyramids refer, because Joseph's body was put ἐν τῇ Σόρῳ. And on this fact alone, if no other could be adduced, the *sepulchral origin* of those monuments is decidedly manifest.

III.

Did any thing occur in the history of the same people, which can be adduced to explain the present violated state of the principal pyramid?

Previous to the consideration of this question, it may be proper to mention, that the custom of heaping an artificial mound, whether of stones or earth, above the Soros, after interment, was a common practice of the ancients. Examples of this kind have been previously alluded to in the former volumes of these Travels. The most ancient form of this kind of mound was not pyramidal.

However

However ancient the Pyramids may be, a simpler hemispheroidal or conical form seems to have preceded the more artificial angular structure. Among the pyramids of *Saccara*, which appear to be more ancient than those of *Djiza*, there are instances, as we have shown, not only of this primæval pile, but of its various modifications until it assumed the pyramidal shape. One example has been noticed among the pyramids of *Saccara*, of an immense mound, which corresponds in its form with the common appearance presented by ancient *tumuli* almost all over the world, as they are found in countries where the pyramidal shape was never introduced. But to proceed in the discussion of the third question.

The body of Joseph being thus placed ἐν τῇ Σόρῳ, and buried according to the accustomed usage of the Egyptians (as manifested by the existence of one of their ancient sepulchres containing the receptacle in question), was not intended to remain in Egypt. The Israelites had bound themselves by an oath, that when they left the land they would "*carry his bones*" with them (Gen. l. 25). Accordingly, we find, that when a century and a half had elapsed from the time of his burial, the sepulchre, which during all this period had preserved his reliques in a Soros, was opened by the children of Israel. Their number amounted to 600,000 men when they went out of Egypt, besides the mixed multitude by whom they were accompanied (Exod. xii. 37, 38); a sufficient army, surely, even for the opening of a pyramid if it were necessary, especially when the persons employed for the undertaking were acquainted with the secret of its entrance; having from the very moment of the patriarch's interment, been under a solemn engagement to remove the body which they had there placed. However this may be determined, it is certain the tomb was opened; for no sooner is their departure mentioned, than we read (Exod. xiii. 19), "*Moses took the bones of Joseph with him.*" Here, then, we have a record in history, which implies the violation of a sepulchre, and the actual removal of an embalmed body from the *Soros* in which it is said to have been deposited. The locality, too, of this sepulchre seems to coincide with that of the particular cœmetery where this pyramid has for so many ages unaccountably borne the marks of a similar violation; its secret entrance being disclosed to view; and its Soros always empty. It is by no means here presumed that this circumstance *will* account for its violated state; but it furnishes a curious coincidence between the present appearance of the pyramid, and a fact recorded in ancient history which may possibly be urged to that effect. No other pyramid has been thus opened; neither is it probable that any such violation of a sepulchre would ever have been formerly tolerated: so

 sacrilegious

sacrilegious was the attempt held to be among all the nations of antiquity, Egyptians, Jews, Greeks, and Romans. At the same time, there are many weighty arguments against the opinion that such a stupendous pyramid would have been erected by Joseph's posterity over his remains, even if they had worshipped him as a God, when it was known that his body was not intended to remain in the country; but the honours paid to the dead in Egypt were, in certain instances, as it is evident, almost beyond our conception; and there is no saying what, in a century and a half, the piety of some hundred thousand individuals might not have effected, especially when aided by the Egyptians themselves, who equally revered the memory of Joseph, although they became, at last, inimical to his descendants. This part of the subject is not altogether essential to the end proposed; it has been introduced rather as a curious inquiry suggested by the connection which appears to exist between the Pyramids and the history of the Hebrews; it neither affects nor alters the main argument.

IV.

Doth any record or tradition attribute the origin of the Pyramids to the Israelites, or to a period equally remote with that of their residence in Egypt?

This brings us to the last article of the inquiry. For the *record* we have only to refer to Josephus, who expressly states it as one of the grievous oppressions which befel the Hebrews, after the death of Joseph, that they were compelled to labour *in building pyramids* (Joseph. Antiq. Jud. lib. ii. c. 9); and the curious memorial, as given by the Jewish historian, is sustained by collateral evidence in the book of Moses. The principal labour of the Israelites is described in Exodus (v. 16) to be a daily task of making bricks, without being allowed a requisite portion of straw for their manufacture. The mere circumstance of 600,000 persons being employed at the same time in making bricks, affords of itself a proof that the building for which these materials were required could be of no ordinary magnitude. This happened, too, after the *death* of one of the kings of Egypt (Exod. ii. 23), at which time, it is said, they began "*to sigh by reason of their bondage.*" It is therefore very probable that the pyramid at which they laboured was the sepulchre of this king: this is a matter of conjecture; although it may be added, that one of the pyramids near Saccara is built of *bricks containing chopped straw.* [It is called *Ktoube-el-Menshieh*, the *bricks of Menshieh.*] The fact for present attention is the record preserved by Josephus, which attributes to the Israelites *the origin of certain pyramids in Egypt:* and for other evidence,

dence, proving them to have existed in a period equally remote with that in which this people inhabited the country, we may refer to the testimony of *Manetho*, whose authority is respected by Josephus, and who, from his situation as an Egyptian priest*, had access to every record preserved in the sacred archives of the country. Manetho affirms that these structures were begun by the fourth king of Egypt during the first dynasty†; which carries their antiquity back to a period earlier than the age of Abraham. Of this nature are the records required by the last question in the proposed inquiry, without having recourse to any of the writers of Greece or Italy. As for the *traditions* which refer the origin of these monuments to the age of the Israelites in Egypt, these exist not only among the Arabians, but also among the Jews and Egyptians. The author of a book entitled *Morat Alzeman*, cited by Greaves in his *Pyramidographia* (p. 6, Lond. 1646), speaking of the founders of the Pyramids, says, "some attribute them to Joseph, some to Nimrod." The Arabians distinguished the Pyramids by the appellation of *Djebel Pharooun*, or *Pharaoh's* mountains ‡; and there is not one of these oriental writers who does not consider them as ancient sepulchres §.

Upon these premises, thus derived from sources that are not liable to the objections urged by Pauw, being wholly independent of any notions with which he supposes the Greeks to have blended their accounts of the Pyramids, the following conclusions may perhaps be warranted:

1. That the Hebrews inhabited Egypt in the period to which the Pyramids may be referred.
2. That the Pyramids contain an existing document corresponding with the mode of interment practised by this people, and were, therefore, intended as sepulchres.

* Josephus says, that the care and continuance of the public records were the peculiar province of the priests (lib. i. cont Apion.). Manetho belonged to the college at Heliopolis, the very seat of Egyptian science. His testimony was preferred by Marsham to that of Josephus himself. However, it should be acknowledged that Perizonius, who considered the dynasties of Manetho as fabulous, attacked Marsham upon this ground, describing him as "*absurdissima quoque Manethonis recipiendi studiosior, quam speciosa Josephi.*"—Vid. Perizonii Ægypt. Orig. Invest. c. xxi. p. 384. L. Bat. 1711.

† "Etenim Manetho jam in dynastia 1. quartum ejus regem, *Venephen*, Pyramides erexisse tradit; ac dein, in dynastia 4. regem secundum, *Suphin*, pyramidum maximam extruxisse." Perizon. cap. xxi. p. 383. This authority, admitted by Marsham, is contemned by the author from whom it is now cited.

‡ See also Egmont and Heyman's Travels, vol. ii. p. 85. Lond. 1759.

§ See the Extracts from *Ibn Abd Alhokm*, and the Arabian authors, as given by Greaves, &c. &c.

3. That the present state of the principal pyramid may *possibly* be owing to the circumstance related in their history, of the removal of Joseph's reliques from the Soros in which they had been preserved.
4. That from the records of Jewish and Egyptian historians, as well as from the traditions of the country, we may attribute the origin of some of the pyramids to the Hebrews themselves; and may assign to others a period even more remote than the age in which this people inhabited Egypt.

In the principal point to be determined, namely, the use for which these structures were erected by the ancients, there cannot remain even the shadow of a doubt. That they were sepulchres has been demonstrated beyond the possibility of a contradiction; and in proving this all the best authorities have long concurred*. In their whole extent from *Djiza* to *Saccara*, the Pyramids, and all their contiguous subterranean catacombs, constitute one vast cœmetery, belonging to the seat of the Memphian kings†, the various parts of which were constructed in different periods of time. Some learned writers however, as Shaw, and the author of Philosophical Dissertations on the Egyptians and Chinese, have exercised their erudition in attempting to prove that the Pyramids were mythological repositories of Egyptian superstitions; and they have described the *Soros*, in direct opposition to Strabo, either as *a tomb of Osiris*‡, or as one of those κίσται ἱεραὶ in which the priests kept their sacred vestments§. Nor, perhaps, would these conjectures have appeared so visionary, if those distinguished writers had carried the investigation somewhat further. If the connection between ancient Egyptian mythology and Jewish history had been duly traced, an evident analogy, founded upon events which have reference to the earliest annals of the Hebrews, might be made manifest. The subject, itself sufficient to constitute a separate dissertation, would cause too much digression; although an endeavour may be made to concentrate some of its leading features within the compass of a note‖. The main object at present is to prove the intention for

* See the authorities and arguments stated by Perizonius, c. xxi. p. 393. Also Greaves's Pyramidographia, p. 43. Lond. 1646.

† Τάφοι τῶν βασιλέων (Strabon. Geog. lib. xvii. p. 1145. Ed. Oxon.) In the threatenings denounced against the Israelites (Hosea ix. 6.) it is said. "MEMPHIS SHALL BURY THEM."

‡ See Pauw on the Egypt. and the Chinese, vol. ii. p. 48. Lond. 1793.

§ See Shaw's Travels, p. 371. London 1757.

‖ Perhaps, with due attention to facts collected from ancient and modern writers, the whole connection might be traced between the history of Joseph, and the Egyptian mythology founded thereon. For this purpose the reader may be referred to all that *Vossius* has written upon the subject (vid. lib. i. cap. 29. tom. i. p. 213. *de Theologia Gentili*: Amst. 1642); who considers

for which the Pyramids were erected; and in this it is hoped we may succeed. If these were the only monuments of the kind belonging to the ancient world, and we had not the evidence afforded by the *Soros* in the principal pyramid, a greater degree of difficulty might oppose the undertaking. But in addition to the testimony offered by this remarkable relique, we are enabled, by collateral evidences derived from other countries, to establish beyond all controversy the truth of their sepulchral origin. It has

considers the Egyptian APIS as a symbol of the *Patriarch*. He supports his opinion by authority from Ruffinus (*Historiæ Ecclesiasticæ*, lib. ii. cap. 36.); and derives evidence from Augustin, (*Script. Mirab.* l. i. c. 15.) to prove that the Egyptians placed an ox near the sepulchre of *Joseph*. It appears also from Suidas (voce Σάραπις), that APIS was by some considered a symbol of JOSEPH: "*Quo ut magis inclinem facit*," observes Vossius, "*quòd Josephus Deut. cap. penult. commate* 17, *bos vocetur, secundùm codices Hebræos.*" But if APIS were the same as JOSEPH, so must also be SERAPIS (or SARAPIS, as it is written by the Greeks) and OSIRIS; for these are but different names of the same mythological personage. "*Factus est Joseph quasi rex totius Ægypti, et vocaverunt eum Apis*," says Kircher (*Œdip. Ægypt. tom.* i. *p.* 196. Rom. 1652); and he gives us from Varro the reason why he was called SERAPIS: "*Quia Arca* (*inquit Varr.*) *in quâ positus erat, Græcè seu Ægypticè dicitur* Σορὸς, *unde* Σοράπις, *quasi Arca Apis, deinde, unâ literâ mutatâ,* Σέραπις *dictus est.*" Also according to Strabo, APIS was the same as Osiris. Ὃς ἐστιν (Ἆπις) ὁ αὐτὸς καὶ Ὄσιρις (lib. xvii. p. 114. Ed. Oxon.) Hence it may be inferred, that as JOSEPH, together with the names of APIS and SERAPIS, also bore that of OSIRIS, the annual mournings which took place in Egypt for the *loss of Osiris's body*, and the exhibition of an *empty Soros* upon those occasions, were ceremonies derived from *the loss of Joseph's body*, which had been caried away by the Hebrews when they left the country. Julius Firmicus, who flourished under the two sons of Constantine, endeavours to explain the reason (*De Error. Profan. Relig.*) why JOSEPH was called SERAPIS. In opposition to the origin assigned by Varro, for the name SERAPIS, it may be observed, that Plutarch (*De Isid. et Osir.* c. 29) derives a notion which prevailed maintaining that SERAPIS was no god, but a mere name for the sepulchral chest where the body of Apis was deposited: Οὐκ εἶναι Θεὸν τὸν Σάραπιν, ἀλλὰ τὴν ἈΠΙΔΟΣ ΣΟΡΟΝ οὕτως ὀνομάζεσθαι. But things which were rejected by the Greeks as inconsistent with their religious opinions, may come much nearer, on this account, to truth and to our own. A very popular opinion has long been entertained, concerning an extraneous idol brought to Alexandria, by one of the Ptolomies, from the coast of Pontus, which received the appellation of *Serapis* upon its arrival in Egypt; but the word *Serapis* is purely Egyptian....and Jablónski has refuted the opinion, by proving that *Serapis* was worshipped in Memphis long before the time of the Ptolomies, (*Panth. Egypt.* lib. ii. c. 5. p. 233. Frank. 1750.) and by showing from Eustathius that the whole story of this Sinopic deity was derived from *Sinopium* near Memphis. Thus Tacitus, "*sedem ex quâ transierit* (*Serapis*) *Memphin perhibent, inclytam olim, et veteris Egypti columen.*" Yet Gibbon seems to imply (*Hist.* c. 28. vol. v. p. 90. Lond. 1807) that both the *name* and the *idol* were alike strangers to the priests of Egypt; and he sneers at the notion of Vossius, that the patriarch *Joseph* had been adored in the country as the bull *Apis*, and the god *Serapis*. (Ibid. see note 36.) The reader may consult the learned observations of Bochart upon this subject (*Hierozoicon*, tom. i. l. ii. c. 34, pp. 345, 346, 347, 348), and also of Jablonski, upon which Gibbon may have grounded his scepticism, although he has not mentioned his authors. The following passage of Apollodorus, as cited by Bochart, proves the name *Serapis* to be of ancient date in Egypt: "*Apis, relatus inter Deos,* SERAPIS *appellatus est.*" Upon the identity of *Serapis* and *Joseph* many learned writers are agreed. "*Sunt qui* APIM *et* SERAPIDEM *unum numen putârint, et per Serapidem* JOSEPHUM *intellexerint*; NEC VERITATI CONTRARIA VIDETUR HÆC OPINIO." (*Cunæus de Repub. Heb. Annot. Nicolai*, c. 17. not. 14. *Thes. Antiq. Sac. Ugolini*, Venet. 1745).

Indeed

has been already shown, that of themselves they constitute but remaining traces of a custom common to all the nations of antiquity. An ancient tumulus for men of princely rank seems very generally to have consisted of *three* parts; the Soros, the Pile or *Heap*, and the *Stélé*. Of these, Homer mentions two at once; as being those parts of a tumulus which were externally visible. As the practice occasionally varied among different nations, only one of these was used to denote an ancient burying-place. In Asia Minor, the *Soros*, of gigantic proportion, sometimes stood alone, without the *Pile* and the *Stélé*. In Scythia and in many northern countries the *Pile* only appears. In Greece perhaps, although no instance is decidedly known, the simple *Stélé*, without the Pile, might serve to denote the grave of a deceased person*. The *Pile* or *Heap* was generally nothing more than a lofty mound of earth: more rarely it was a magnificent pyramid. A square platform was left, in some instances, upon the tops of those pyramids, as a pedestal for the Stélé.... Hence originated the appellation of *Hermetic Stélæ* (because Hermes had the care of the dead), and all the Grecian mythology connected with them†. In America pyramids were built in this manner by the ancient inhabitants of that great continent. That those pyramids were also temples is true; because all ancient sepulchres were objects of worship, and tombs were the origin of temples.....By the account which Gmelli gives of the Mexican pyramids at *Teotiguatan* (*a place of Gods*, or of *Adoration*), they were erected, like the Egyptian Pyramids, for sepulchres. The first he saw was the pyramid of the Moon, about 150 feet in height. "It was made," he says, "of earth, in steps, like the Pyramids of Egypt;" and on the top of it was a great stone idol of the *Moon*. The pyramid of the *Sun* was

Indeed the number of authors and commentators by whom this opinion is maintained may be considered as more than a counterpoise to the objections of Bochart and of Jablonski. Tirinus (*Annot. in Sulpit. Sever.* p. 59. Ed. Horn. L. Bat. 1654) in addition to the authorities above cited mentions also Pierius and Baronius: and he further observes, "Idque patet, tum ex nomine *Serapis* quod Bovem notat; tum ex nomine *Arsaph*, quo, teste Plutarcho, Osiris vocabatur, levi commutatione ex *Joseph* facta: tum ex Hieroglyphicis, quibus Osiridem designabant, puta figura bovis seu vituli notis Lunæ et Solis insigniti: item juvenis imberbis cum modio et calatho in capite. Quæ in Josephum, ejusque boves et spicas, et ætatem, et astrologiæ peritiam, ad amussim quadrant. Subscribitur Clemens Alexandrinus, Augustinus, A. Lapide, et Bonfrerius." See also *Spencer de Leg. Heb.* lib. iii. pp. 270, 271. *Beyer, Hen. V. Veghorst. de vero Dei Cultu, p. m.* 25. *edit. Kilon.* 1671. *Michael.* Not. ad Gaffarell. Curiositates, edit. Hamburg. &c. &c.

* *Καὶ Στήλην ἐπ' αὐτῷ γενέσθαι, οἷα νεκρῷ.* Clem. Alex. lib. v. The great column at Alexandria, called "*Pompey's Pillar*," may possibly be an example of the *Stélé* standing *alone*.

† A *dog* is often represented upon the sepulchral Stélé as a type of the Egyptian Mercury. This deity appears, upon Egyptian monuments, represented as a human figure with a *dog's head*.

about

about 40 feet higher, and upon the top of it a vast statue of the *Sun*.....Within the pyramid of the Moon were vaults *where their kings were buried;* for which reason the road to them is called Micatoli, that is to say *The Way of the Dead*. Precisely too after the manner in which the Pyramids of Egypt are surrounded by sepulchres of a more diminutive form, the Mexican Pyramids have, as Gmelli tells us, "about them several artificial mounts, supposed to be burying-places of lords." Another instance, and more remarkable for the similitude it bears to the principal pyramid of Egypt, is the great pyramid of Papantla mentioned by Humboldt, in which *mortar may be discerned in the interstices between the stones*. It is an edifice of very high antiquity, and was always an object of veneration among the Mexicans.

LII. *Further Queries, as to the proper Places in the British Series of Strata, of the Newcastle Grindstone Rock and its Muscle Shell Ironstone, and of certain organized Remains found near Cambridge. By a* Correspondent.

To Mr. Tilloch.

Sir,—My grateful acknowledgements are due to N. J. Winch, Esq. for his prompt and kind attention in p. 233, to a part of my Queries in p. 108 of your Magazine. It seems now I think, that the "bivalve shells" of Mr. W. are the *Myæ ovatæ* of William Martin, and that the "Grindstone sill" of W. Forster (fathom 244 to 247) is a far lower stratum than the Gateshead-fell Grindstone sill: but I am still left in ignorance, of the particular strata to which these Myæ and Newcastle Grindstone Quarries, are to be respectively referred? and I presume therefore to prefer these further requests to Mr. W. or any other kind friend of Geological investigation: requesting, in order to avoid mistakes, that they will mention the Name of the stratum and *Fathom* on Mr. Forster's attached scale, in each instance.

In wishing further reference to *Mr. Forster's* Section to be made, I beg not to be understood by Mr. W. as countenancing plagiarism, or as wishing to keep out of honourable view, the names of those deserving individuals *Hutchinson*, *Johnson*, and *Miller* (Millot must be a misprint), who are said to have first published the detached details of the Newcastle Coal strata; but merely prefer Mr. Forster's work, as being the only *collection* of these valuable materials, *in a portable form:* and I shall feel

highly obliged to Mr. Winch, or any other gentleman who is possessed of the Book and the two engraved sheets mentioned, who would send to you exact copies of these sinking accounts, with such introduction and descriptive matter concerning them, dates of publication, &c. as may effectually secure to their authors the future credit of them, when recorded in your useful depository of matters of fact regarding the British stratification.

On looking through your last number, and that of the work in which Mr. Winch originally wrote, I regret, not to find any of the requested information from the respectable keeper of the Woodwardian Museum at Cambridge, as to whether "charred wood, mutilated fish, pentacrinites, &c." are rarely found in that neighbourhood, *in* the undisturbed *substratum* to the hard or flintless chalk (whether named Chalk-marl or Galt, is very immaterial), or whether some of these Reliquia have not been taken from the superficial *alluvial** Clay?. I presume however again, respectfully to prefer the above request to the Rev. Mr. H., or to any other of the numerous friends of science within the walls of Cambridge, and hope that some of these will favour me and your Readers in general with a full elucidation on this point; and which will greatly oblige

A Constant Reader.

* In another communication which I sent you a few days ago, recommending a more general research after *Fossil Shells*, I have endeavoured to give those distinguishing marks, by which *Mr. W. Smith* and those who are treading in his steps in the minute examination of the British strata, are enabled to distinguish the *alluvial shells* and other organic Remains, from those lodged *in the strata*, and to show the essential importance of these discriminations.—Having mentioned Mr. Smith, permit me to add here, that I have very lately seen stuck up in Mr. Cary's window, No. 181, Strand, a List of more than 380 *Subscribers* to Mr. Smith's long-expected "Delineation of the *Strata* of England and Wales," &c. with an assurance that the same will very soon be issued to them. If on perusing this respectable List of Names, it should be found almost entirely barren of names familiar to us of late years, as managers of the Geological, Wernerian and other *Societies*, or writers in their Transactions, professing to have *the same objects in view*, as stimulated Mr. S. to commence his labours 22 years ago, this circumstance will not I am sure weigh with or induce any Gentlemen of true British feelings, and who are well wishers to the progress of knowledge, longer to withhold their patronage to a work, of British origin, and promising great national advantages in the increased knowledge and improved working of our Mineral treasures.

LIII. *Mr.*

LIII. *Mr.* Bakewell *in Reply to Mr.* Frere; *and on some peculiar Properties of Light.*

To Mr. Tilloch.

Sir,—A Correspondent in your last Magazine, who signs himself J. H. Frere, has been pleased to insinuate that the sketch representing the arrangement of the strata from the German ocean to the Irish channel, given in the preceding number, was taken from a section he had previously made and sent copies of to the Rev. W. Turner and Mr. Wynch, of Newcastle, and to Mr. Greenough, in London. I trust I shall be allowed sufficient space to repel the accusation, though it is with regret that I occupy a part of your pages with subjects of a personal nature.—Until I saw the letter of Mr. Frere's, I had never before heard either of that gentleman or his section; but I immediately wrote to Mr. Turner for an explanation, who informs me that some months after I returned from Newcastle, Mr. Frere did send a section there; but I can confidently appeal to Mr. Turner, Mr. Wynch, or any person in that place, that I never received from them the slightest notice of its existence. I can make the like appeal to Mr. Greenough; and as I have not been at the rooms of the Geological Society for more than two years, I have never seen it there, nor have I heard of it from any of the members; (it may be observed that the Society has not thought proper to give it a place in their recently published volume of Transactions;) nor can Mr. Frere name any other person who has either directly or indirectly acquainted me with his section. I believe Mr. Farey and many other gentlemen interested in such subjects were equally ignorant of Mr. Frere's section. The sketch sent you was laid down on Cary's large Map of England, which your engraver reduced. I did not attempt more than a general resemblance of the surface outline. In one now engraving for the second edition of my Introduction to Geology, I have endeavoured to preserve the *contour* of the principal mountains. What coincidence either of these may have with Mr. Frere's section, I am to this hour unacquainted with; but if two delineations of the same object laid down on the same ground-plan, and where the characters are so strongly marked, have not a near resemblance, one or both must be manifestly erroneous. The part between Cross Fell and Burtreeford Dyke was drawn from Mr. Forster's description: all the other parts were the result of my own observations and inquiries. In my Introduction to Geology in 1813, I described several sections across England, and in my Lectures at the Russell Institution in February 1812, I exhibited a section of the rock formations between Hull and Liverpool.

Liverpool. Mr. Frere might with equal justice say that these were also copied from his labours.

In the last number of the Monthly Magazine, an anonymous writer has repeated the same charge respecting Mr. Frere's section, mixed up with some illiberal reflections on Dr. Thomson and myself, for having published An Account of the Geology of Northumberland and Durham. We have indeed been guilty of regretting the too great apathy respecting the destruction of human life by explosions in the mines; and if this have given offence, I am willing to bear my share of the censure: but with respect to the publication of information given by others in an improper manner, I utterly disclaim the charge as false. The only one who could have the slightest grounds for complaint, is the gentleman who I since understand has written a paper on the same subject (Mr. Wynch); but I have his assurance communicated to a mutual friend, that he has not now, nor ever had, the least reason for complaint against me for what I have published.

To return to Mr. Frere. From his letter it might be inferred, that after he had made his section, he put the mountains into his pocket to prevent any one from attempting the same thing. If he has done so, let me entreat him to place them in their old station again, and content himself with threats of pains and penalties against all future drawers of plans and sections. The observations from which my section was framed, were, however, made previously to Mr. Frere's monopoly of the mountains; I ought not therefore to incur his displeasure.

Mr. F. says, If my section were not copied from his, he must place himself in the degrading situation of having copied from me. This is a dilemma of his own choosing; but I may tell him without fear of contradiction, that he has placed himself in the more unpleasant situation, of having brought forward an accusation without any evidence whatever to advance in its support. From this difficulty Mr. Frere can only extricate himself by candidly admitting that he has fallen into a mistake (perhaps not very uncommon in life), namely, that of supposing himself and his labours to be better known than was really the case.

In what I have stated, I by no means wish to be understood as disparaging Mr. Frere's section, which I have not seen. From his residence in the country, it is reasonable to presume that it may be a more correct representation than the one I have given; and I hope Mr. Frere will favour the public with an engraving and description of it: I can assure him there is no one to whom the publication will give greater pleasure than to myself.

I am, sir, yours, &c.

R. Bakewell.

13, Tavistock Street, Bedford Square,
April 15, 1815.

P. S.—The very ingenious experiments of Captain Kater (in the last volume of the Philosophical Transauctions) on the different quantities of light reflected by the Gregorian and Cassegrain telescopes with specula of the same aperture, appear to prove that when the rays of light converge to a focus, a considerable part of them are (as far as affects the sight) extinguished before they diverge again. I do not know whether similar experiments have been made with refracting telescopes, with a view to confirm this fact; but there is one case well known to the earliest optical writers that has hitherto been unexplained, but which I conceive offers a full and decisive confirmation of the same phænomenon. In the original telescope of Galileo with a concave eye-glass, the rays are intercepted before they come to a focus; and though this construction was laid generally aside on account of the small field of view; yet such is the superior brilliancy of the effect, that Jupiter's satellites may be seen with a common refracter of twenty inches in length, with a concave eye-piece, more distinctly than with one of four feet when a convex eye-glass is used. Having when a school-boy amused myself with the construction of telescopes of this kind, I was well acquainted with the effect: on referring to optical writers for the cause, I was informed that it arose from the concave glass being much thinner; and by later writers, that the effect was produced by the flint glass of the eye-piece correcting in some degree the aberration from the different refrangibility of the rays, as in the achromatic object glass. Without denying that both the above causes may contribute something to the superior brilliancy of the object, I am persuaded that the solution which is offered by Capt. Kater's experiments is the only one that will fully account for the different effects of the convex and concave eye-glasses in refracting telescopes. It is much to be desired that Capt. Kater, or others who have leisure for such experiments, would prosecute these inquiries, as they cannot fail to contribute much to the improvement of optical instruments. They will also elucidate one of the most interesting subjects of philosophical investigation, respecting the effects produced by the rays impinging on different surfaces, as well as on each other under different angles, and the still more extraordinary effect of their impact on each other as they converge to a focus. Those who have attended to the experiments of M. Malus and Dr. Brewster will immediately perceive the importance of Capt. Kater's discovery in reference to the phænomena of the polarisation of light. If the particles of light have each a positive and negative or attracting and repelling pole, as certain experiments appear to indicate, may not the opposing poles of some of the particles be brought so near to each

each other, in crossing at the focal point of a lens or speculum, as to be neutralized, and thereby lose the property of acting upon the retina? R. B.

LIV. *On Coal Formations. By* W. H. Gilby, *Esq.*

To Mr. Tilloch.

Sir,—The following remarks have originated from a paper of Mr. Farey's in the last number of the Philosophical Magazine, respecting the coal formations in North Wales, and the relation of the red ground to the coal and its accompanying strata. From the general tenor of this paper, Mr. Farey seems to claim the credit of having first brought forward to the notice of the public, the fact that the different beds of the red ground so widely distributed through many parts of England, lie in an unconformable position over the inclined coal measures. If that gentleman had consulted a description of the neighbourhood of Bristol, which you did me the favour of inserting in one of your late numbers, he would have found that this circumstance had been then most explicitly detailed. The arrangement which for the sake of method and clearness I followed in that paper was entirely founded upon this very circumstance. For as the formations in the neighbourhood of Bristol consist either of inclined strata or of such as lie "unconformably in an horizontal position upon the tops of the inclined strata;" I began by describing those of the first description, comprehending the inclined coal measures, the mountain limestone, and other rocks which dip under them. I then proceeded to treat of the *unconformable horizontal series*, beginning from the beds of the red marl and passing upwards to the lyas, &c. As this account was printed in a journal which Mr. Farey is in the habit of making the repository of his *practical* observations, I cannot suppose that he was unacquainted with it; and as he has alluded to no *writer* who had noticed the fact before him, he plainly wishes to appear as the first announcer of it. It is very evident, therefore, that Mr. Farey has acted in an unfair and disingenuous manner in manœuvring for himself a gloriola, which he does not deserve; and whatever consequence may be attached to the observation,—and I clearly see that it is of considerable importance to coal proprietors,—I shall at least be entitled to the priority of having ascertained it before Mr. Farey, inasmuch as my paper was printed full half a year before his.—I cannot suppose that Mr. Townsend is ignorant of the relation which the red ground bears to the coal measures; but,

but, I believe, it would not be very easy to collect the fact from his writings. As far as I yet know, my description of the formations in the neighbourhood of Bristol contained the first and most distinct information as to the unconformable position of the beds of the red ground over the inclined coal measures; and at this moment it is peculiarly gratifying to me to state, that Professor Jameson, while lecturing during the course of last week upon coal, did me the honour to allude to my paper, and at the same time mentioned me, as having first observed the real position of the red ground, in regard to the coal and its accompanying strata.

As Mr. Farey plumes himself so much upon his *practical* knowledge, and as he seems to have seen so much of the red ground, I cannot but be surprised that he has been so long in the dark as to the beds of the red ground overlying the tops of the inclined strata. This formation must have very different characters in the country where that gentleman has seen it, from those which it exhibits in the district which has been the subject of my examination. There the most incurious observer could not but remark, in very many places, the beds of the red ground, that is to say, the coarse breccia, the red calcareous sand-stone, and sometimes a loose clay-stone abounding with gypsum, occurring in an unconformable situation, sometimes over the coal measures, and at other times over the inclined rocks that inclose the coal tract. These remarks, I believe, will be sufficient to show that Mr. Farey has claimed what he has not a fraction of a right to; I shall therefore conclude by subscribing myself,

Sir,

Very respectfully yours,

W. H. Gilby.

Surgeons Square, Edinburgh,
April 12, 1815.

P. S.—Want of leisure has prevented me from sending you some additional remarks upon the stratification in the vicinity of Bristol, as also some observations respecting the red ground, the red sandstone, and the point to which, in the Wernerian system, the Bristol formations are to be referred. These I hope soon to submit to you.

LV. *Mr.* Hume's *New Process for Emetic Tartar.*

To Mr. Tilloch.

Sir,—About two years ago, I was requested by some members of the Royal College of Physicians, to turn my thoughts to the

the preparation of *Emetic Tartar*, and more especially to such processes as would admit of the common *black sulphuret of antimony* being employed.

Having succeeded in my endeavours to attain this object, and received the most ample testimonials from my friends and many other practical chemists, of the perfection, practicability, and, saving in respect to expense, by following this process, I shall on the present occasion, just mention in general terms the prescription itself, and then leave any observations I may have to offer on the subject, or on the labours of others who have preceded me, for another opportunity.

Antimonium Tartarisatum.

Two parts of black *sulphuret of antimony* in fine powder, and one part of *nitrate of potash* are to be mixed and added to two parts of *sulphuric acid*, previously mixed with eight parts of water and suffered to cool. By a due application of heat a proper oxide of antimony will be formed, which when thoroughly washed is to be boiled, while yet moist, with two parts of *supertartrate of potash* and a proper quantity of water. The solution is then to be filtrated, evaporated, and treated after the usual manner for crystallization.

Although I succeeded by other methods, peculiarly my own, to prepare this very important medicine, yet, upon the whole, I am disposed to prefer the formula which I now lay before you, more particularly as it is sanctioned with the decided approbation of those whom I wished to serve and consult.

I have here adopted the nomenclature of the College, which, after all, is as correct and convenient as any other that has been proposed for this article. I profess, indeed, to be an advocate for many of the arbitrary names, especially for such bodies as are beyond a binary composition; and, where the ingredients enter, as in emetic tartar, alum, Rochelle salt, &c. in *definite* proportions, the rule is perfectly admissible; I should propose, therefore, to retain both these preparations of tartar, at least under their present titles, viz. *antimonium tartarisatum* and *soda tartarisata*.

I remain, sir,

Your obedient servant,

Long Acre, April 14, 1815. Jos. Hume.

LVI. *On*

LVI. *On Carbonate of Ammonia as a Manure.*

To Mr. Tilloch.

Sir,—I would beg leave to recommend to your agricultural readers the use of the liquor produced in the distillation of coal, at the Gas Light Establishments, as a valuable manure. It contains principally carbonate of ammonia; and this, according to Sir H. Davy in his Elements of Agricultural Chemistry, quarto, page 297, was found to produce more luxuriant effects upon some plants on which he tried it, than any other saline solution he made use of. And he very justly observes, that this result might be expected; for carbonate of ammonia consists of carbon, hydrogen, oxygen, and azote, the principles of which all vegetables are composed. But from the circumstance of this liquor containing a little sulphur, there is another valuable property it possesses as a manure. If it be diluted in the proportion of one gallon to three of water, and then applied, it will have the effect of destroying insects and grubs which frequently prove so destructive to growing crops. In the proportion of one gallon to sixteen or eighteen of water, it may be applied to all green crops with effect.

The tar that is produced in the same operation, when mixed very largely with soil in the proportion of a gallon to about a wheelbarrow of mould or marl, and this mixture used as a compost and ploughed in, or applied as a top dressing, will also be found a most valuable and active manure.

I remain, sir,

Your most humble servant,

April 17, 1815. C. D.

LVII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

April 6. A paper by J. Knox, Esq. was communicated by Dr. Young, detailing a series of experiments on coloured concentric rings, by means of lenses, on a piece of plain mirror, or on a table before a window. It appears, according to these experiments, that the thickness and number of glass plates produces the variety of coloured appearances. Thus, a piece of common crown glass laid on a table opposite a window, and a wire rod held over it, the shadow is seen double; with two pieces of glass placed on each other in the same manner, the shadow appears treble, with three pieces quadruple, and so on; shadows always increasing according as the reflecting and refracting surfaces increase.

crease. By using a lens instead of a flat glass, coloured fringes are seen: with the first lens the colours appear in a line; when the second lens is added to it, the fringes of colours then assume a circular form; when a third is applied, a new series of coloured fringes appear, which the author calls intersectionary fringes. Coloured rings also appear in these experiments, the same as in Dr. Herschel's researches. Mr. K. used a black card, the same as Dr. H. He also painted the under side of his glasses, in order to prevent the appearance of coloured concentric rings, and the better to observe the nature and modulations of the fringes, which had not been observed, by preceding experimenters, and of which he endeavoured to make tolerably accurate drawings. The author very modestly avoids drawing any hasty general conclusions from his experiments, but seems to think that reflection and refraction are sufficient to explain all the phænomena of coloured concentric rings, by the solution or separation and combination of rays without any intervening plate of air, or fits of easy transmission, as supposed by Newton. He found that in vacuo, in water, or in air, the same rings were produced; but nitric and other heavy acids destroyed both the rings and colours.

Baron Von Humboldt of Berlin, and Messrs. Biot and Gay Lussac of Paris, were severally elected this evening (being the first meeting after Easter) foreign members of the Society.

April 13. The veteran Major Rennel furnished another long paper on the subject of currents in St. George's Channel, in addition to and illustration of his former paper published above twenty years ago (1793) on this subject. He chiefly dwelt on a NW current proceeding from Cape Finisterre to Cape Clear and the Scilly Isles; his facts were taken from old journals kept on dead reckonings in 1737, and some books on navigation published in 1755. He also mentioned the experiment of a bottle being carried by a NE current to Cape Ortegal, &c.

April 20. Part of a very curious and important paper by Sir H. Davy was read, detailing some of his new and curious experiment, and discoveries on the combinations of iodine with oxygen and the acids. It appears that when oxygen is combined with iodine, it first assumes a pastey or gelatinous appearance, and afterwards becomes solid; with sulphuric and nitric acids something similar occurs: these solid combinations Sir H. very properly calls oxyiodats, as they differ very materially from all the other salts the names of which terminate in *ate*. Gay Lussac, however, having anticipated the existence of such bodies, proposed to call them iodates; but this name, Sir H. justly observes, conveys a very erroneous idea of their real character, which is singular and interesting. The colour of these salts approaches to violet, according to the quantity of uncombined iodine present in them.

ROYAL

ROYAL INSTITUTION.

Mr. Brande's seventh lecture related exclusively to the discoveries of Mr. Cavendish, and to the state of chemical philosophy at the period at which he terminated his successful career.

Although Van Helmont, Mayow, and others were acquainted with inflammable gaseous fluids, the nature of the principle upon which their inflammability depended had been but very imperfectly guessed at, till Mr. Cavendish turned his mind to the subject, and published upon it in the Philosophical Transactions for 1766. The nature and properties of inflammable air are there dwelt upon with a masterly perspicuity, and its leading peculiarities particularly developed.

That the combustion of a mixture of this air with common air produced moisture or dew, had been observed by Macquer, Priestley, and others; but its source was hidden until exposed by the acute suggestion of Mr. Watt, and the decisive experiments of Mr. Cavendish, who burned by means of the electric spark a mixture of two measures of inflammable air, and one of dephlogisticated air, and obtained an equivalent weight of pure water: thus was the composition of this universal fluid (till then deemed elementary) demonstrated with an exactness and precision of which chemists had hitherto had no example.

Mr. Brande said, that the subject upon which he was then engaged was of such consequence in the history of the French chemical revolution, of which he should consider the merits and demerits in his next lecture, that it would be proper to exhibit a few analytic proofs of the correctness of Mr. Cavendish's general views. The decomposition of steam by red hot iron, and of water by the galvanic battery, were then shown.

Several singular properties of *hydrogen*, or inflammable air, were illustrated; especially the production of musical tones by the combustion of the gas in tubes of different diameters and materials.

Having noticed Mr. Cavendish's second great discovery, that of the composition of nitric acid, the Professor entered at some length into the discussions which had arisen concerning the identity of hydrogen and phlogiston, and showed, by several apposite experiments, the additional strength and probability which the phlogistic hypothesis had attained in consequence of the discovery of inflammable air: he said that, in treating of the antiphlogistic doctrines, he should draw a comparison between the facts upon which they are founded, and those which support Stahl's notions. Having put his audience in possession of Mr. Cavendish's principal discoveries, Mr. Brande digressed shortly

 upon

upon the peculiar genius and talents of the discoverer: he especially praised the mathematical precision and statical accuracy which glitter in his dissertations. He should not endeavour to draw a parallel between the philosophical character of Mr. Cavendish, and that of his fellow labourers in science, for he admitted not of comparison; "he was himself alone."

Towards the conclusion of his lecture the Professor summed up the additions which the labours of Black, Priestley, Scheele, and Cavendish, had made to the stock of chemical knowledge; the elements of the ancients had vanished before them; air and water had each been decompounded, and had yielded new elements; their properties had been investigated, and the inquiry had thrown an entire new light upon the science: such were the favourable auspices under which Lavoisier and his associates entered the field of chemical philosophy.

Mr. Brande in his eighth lecture resumed his History of Chemical Philosophy with the discoveries of Lavoisier and the views of the French School. With Lavoisier, as with his predecessors in the field of theoretical chemistry, the phænomena of combustion were a leading object of attention, and Black's theory of latent heat was assumed as the groundwork of his new views. Oxygen was considered as the only supporter of combustion; in its gaseous state it was regarded as a compound of a peculiar basis, combined with light and heat; during the process of combustion the ponderable basis united to the burning body, while the light and heat were developed in the form of flame. Oxygen, as its name imports, was also looked upon as a necessary constituent of all acids—it was described as the universal acidifying principle.

Having illustrated these views by experiment, and enlarged upon their applications by the French chemists, the Professor proceeded to notice the weak and faulty parts of the theory which had been raised upon them. In regard to the evolution of heat and light depending upon the transition of oxygen from the gaseous to the solid form, Mr. Brande remarked, that it often took place during the change of that principle from its solid or liquid, to its aëriform state; as in the combustion of gunpowder, and of oil of turpentine and nitric acid; that the result of combustion in oxygen gas was often itself gaseous, and that numerous substances burned with great energy and splendour, independent of the presence of aëriform matter: this was shown by several experiments; the most striking was the vivid combustion of phosphorus, on being brought into contact with iodine in an exhausted receiver. As to the necessary presence of oxygen in acids, the Professor remarked, that although many acids

acids contain it, there are many without it; and that it exists in the alkalies and in the earths: this was well illustrated by the combustion of sulphur in oxygen, which produced sulphurous acid: the acid thus formed was then transferred to a retort containing potassium, which was made to burn in it, and thus generate an alkali: oxygen was therefore exhibited alternately as an element of an acid, and an alkali.

Hence we are to conclude, in direct opposition to the tenets of the French School, that acidity is not dependent upon oxygen, but upon the basis it unites to; and that combustion may be equally independent of oxygen, and must be considered as a result of powerful chemical action, or other violent motion of the atoms of bodies, not as ensuing from the mutual agencies of any *appropriate principles.*

The identity of charcoal and the diamond was one of the most singular and important of Lavoisier's original discoveries: he had shown that both produced fixed air or carbonic acid during combustion; and it was since supposed to have been proved that the quantity of fixed air generated by burning the diamond in oxygen, was exactly equal to that obtained from the same weight of charcoal. The Professor said that the most eminent chemists had been engaged in this inquiry; he exhibited their different modes of experimenting, and noticed particularly the elegant researches of Mr. Tennant, whose loss society has lately had to deplore.

Mr. Brande concluded this part of his subject with the investigations instituted by Lavoisier to ascertain the nature of the atmosphere, and the proportions of its component parts, and terminated his lecture with a short account of the character of that philosopher, and of his melancholy death during the violence of the revolution in 1794.

" By some," said the Professor, " he has been extolled as the most towering genius of his age, by others stigmatized with the dishonours of plagiarism; but these are the extremes of panegyric and malevolence, each equidistant from candour and truth. His merits were distinct and peculiar; but those who have censured him with the uncandid appropriation of the thoughts and discoveries of Rey and Mayow, of Black and of Priestley, have unquestionably some grounds for the accusation. Let us remember, however, that Lavoisier was never fairly confronted with his rivals and antagonists; that unintentional inadvertency often hangs upon scientific ardour; that in the eagerness of pursuit he may have neglected that which in a calmer hour he would have seen, regretted, and acknowledged; and that in the hurry of discussion, and heat of controversy, he was summoned to eternity with all his imperfections on his head."

 KIRWANIAN

KIRWANIAN SOCIETY OF DUBLIN.

The reading of the "Essay on the Origin, Progress, and present State of Galvanism, with Observations on the Inadequacy of the Hypotheses, &c. &c." by M. Donovan, Esq. Secretary, was continued.

April 5. Examination of the hypothesis of Volta.—An arranged statement of this philosopher's opinions was first given, with the fundamental experiments on which these opinions were grounded. The principle of the disturbance of the electric equilibrium was first discussed. It was conceived that, for reasons assigned, this principle is repugnant to the known laws of electricity; that it is not supported by any of Volta's experiments but one, and that this lies under a two-fold objection: first, it admits of a different explanation; and next, it has not succeeded in the hands of others. The structure of the pile apparently affords a proof that contact produces electricity: but experiments were adduced, which appeared to prove that during the contact of the plates there is no electrical appearance, that these are produced before the contact takes place, and after it is broken; or, in other words, that the evolution of electricity happens when the plates are at a minute distance asunder. The experiments of Wilson proved that during contact the electrometer is not affected: new experiments were detailed, showing that metallic electricity obeys the common law, and that contact of conductors instead of disturbing restores the equilibrium. The effects of placing a metal between two others of a similar kind were next examined. Volta conceives that two electric currents are excited in opposite directions, which therefore destroy each other: hence, in such a situation of the three metals, the equilibrium is maintained, and accordingly the electrometer is not affected. The author showed, that the three metallic pieces thus arranged display phænomena which do not belong to these metals under ordinary circumstances; and, therefore, since the electricity is in a state of repose, what is the cause of the effects. A great variety of other considerations were adduced, from the whole of which the precise state of the question concerning the agency of contact was concluded to be as follows. The principle is opposed by the laws of electricity: it is founded on experiments which admit of a different explanation, which were made with a doubtful apparatus, which did not afford similar results when instruments less liable to fallacy were employed, but in some cases entirely opposite: it is proved that contact instead of disturbing restores the equilibrium; and that some phænomena of contact are inexplicable by this principle, whether the electricity be in action or at rest.

Waving

Waving the objections to this part of the hypothesis, a trial was made how far the phænomena of galvanism are explained by the principle in question, supposing it true. A series of arguments were adduced, tending to show that the increasing positive and negative states of the pile towards the top and bottom are not made necessary, but rather counterindicated by the hypothesis: and that contact with both ends of the pile ought not to be necessary in order to receive the shock. Experiments made with a pile of a thousand plates of highly polished zinc and copper plates were detailed, the results of which were considered to prove that the electric states of the extremities are very different from what has been supposed by Volta and others. The effect of the saline solution interposed in the pile was shown to have little relation to its conducting power, and the efficacy of oxidation of the plates, as supposed by Volta, was considered highly improbable: but this part of the subject was deferred for a future discussion.

II. Examination of the hypothesis of Fabroni.—This hypothesis is directly opposed to that of Volta: in the latter, electricity is supposed to be the cause, and the chemical phænomena no more than effects: in the former, the chemical action is conceived to be the cause, and the electrical appearances are considered effects. That the effects called galvanic are always produced in consequence of chemical action, and never else, the author conceived to be a true proposition in its fullest extent; and experiments to prove that there is not one militating instance, had been already laid before the Society in the statement of a new theory of galvanism. The other principles of Fabroni's hypothesis it was considered unnecessary minutely to investigate, as several facts and arguments adduced were plainly irreconcileable to them. The consideration of the main proposition of this hypothesis, it was observed, came under discussion in a more advanced period of the essay.

III. Examination of the medium hypothesis adopted by the British philosophers.—Drs. Wollaston and Bostock were the first who attempted to reconcile the two preceding hypotheses: with Volta they considered electricity the agent, but with Fabroni supposed that this electricity was evolved in consequence of chemical action. Sir H. Davy proposed a further modification. He conceives with Volta, that the contact of the metals is the primary exciting cause of the evolved electricity; that electricity is the agent in galvanic phænomena; but with Fabroni he allows that chemical changes are essential to the continued action of the pile. The disturbance of the equilibrium by contact, it was observed, lies in this hypothesis under the same objections as in that of Volta. The exaltation of the two states of electri-

city in the galvanic series, attributed by Sir H. Davy to *induction*, was conceived to be not founded upon any known law of electricity: in all cases of induction, the intensity induced is never greater than that of the inducing cause; and this was illustrated by various experiments. Numerous objections were made to this explanation; amongst others it was stated that the principle of induction is unnecessary, that it cannot operate, that if it did it would not explain the phænomena, that it excludes the state of electric neutrality known to exist in the middle of the pile. It was also objected, that although the hypothesis conceives the separation of the differently electric elements of the saline solution interposed in the series to depend on the difference of electric state in the metals, yet the same separation is effected in cases where experiment proves that negative electricity is not present. Considerations were also adduced, which seemed to show that neither positive nor negative electricity acts any part in the separation of the combined elements.

In the hypothesis of Davy, water is assumed to be an insulator: and upon this question rest the claims of the whole. Various experiments were stated, designed to prove that water is a very perfect conductor of low intensities, and whenever it is an imperfect conductor, that it is only to the highest intensities. But whether water be or be not an insulator, the result was supposed to be equally unfavourable: if its insulating power be denied, every principle of this doctrine is subverted; if admitted, it opposes some of the facts which it was intended to explain. Various other imperfections of Sir H. Davy's views were noticed; and this part of the essay concluded with some general remarks on the contradictory nature of the fundamental principles of galvanic hypotheses.

[To be continued.]

FRENCH INSTITUTE.

[Continued from p. 232.]

M. Augustus St. Hilaire, whose papers on Botany have been frequently mentioned, has this year made some observations on several families of plants, in which the *placenta*, i. e. the part of the fruit to which the seeds adhere, is simple, and placed in the midst of the fruit like a column on an axis.

When the summit of this column is free, the route by which the influences of the pollen are transmitted from the pistil to the seeds seems to be sufficiently complicated, and is formed by vessels which creep along even the sides of the fruit, in order to penetrate into the placenta by its base, and proceed to the seeds side by side with the nutritive vessels. Such is in fact the progress

gress of these vessels in the *Amaruntaceæ*, according to M. St. Hilaire; but this observer has remarked that in most of the plants of the class at present under his observation, and particularly in the *Primulaceæ*, the *Portulaceæ*, and the *Caryophylleæ*, the fecundation is produced in a more direct way; and for this purpose there exist originally very slender vessels proceeding from the basis of the style to the summit of the placenta. These threads are destroyed after fecundation, and it is then only that the summit of the placenta becomes free.

M. St. Hilaire also adopts, as constant, the existence of a point or of a pore different from the umbilicus, by which the fecundating vessels arrive at the grain, and to which M. Turpin, as mentioned in a preceding report, has given the name of *micropilis*.

The purely botanical part of M. St. Hilaire's memoir presents many observations in detail, unfortunately very little susceptible of analysis, on the particular characters of certain plants of the families which he has examined, some of which seemed to him to serve as the types for new genera, and others ought to pass into families different from that in which certain imperfect observations had hitherto placed them.

The pisang, banana, or fig-tree of Adam is a herbaceous plant, of the height of a tree, very remarkable by the enormous extent of its leaves, and celebrated for the utility of its fruits, which furnish the inhabitants of the torrid zone with one of the principal articles of their food. Cultivation has multiplied its varieties to such an extent, that there are as many kinds of it as of apple or pear trees.

M. Desvaux, who has collected all that has been written on the banana, has reckoned forty-four varieties in the common species, or the *musa paradisiaca* of Linnæus, and three species distinct from that: viz. the *musa sapientum* Linn., the *musa coccinea* now quite common in Europe, and the *ensete* described by Bruce in his Travels.

The fig-tree has undergone still more numerous modifications in the hands of cultivators. Marquis Suffrein, who resides in Provence, which is so famous for its figs, has taken the trouble to enlighten cultivators as to the true method of bringing the fig-tree to perfection. He has already made known 162 varieties, and his researches are not yet terminated.

M. Thiebaut de Berneaux, who purposes to give a French translation of the works of Theophrastus, and who, in order to ascertain with more accuracy the vegetables mentioned by this celebrated successor of Aristotle, has performed several journeys in the countries where these vegetables grow, and has presented to the Class some of the results already obtained, not only in the

species indicated by Theophrastus, but also in those mentioned in the other Greek and Latin authors.

Thus the *chara*, which Cæsar's soldiers discovered so fortunately under the walls of Dyrrachium, and the roots of which prevented them from starving, deserves well to be sought after. At present this name is given to a small aquatic plant, which certainly can afford nourishment to no person; and there are a multitude of contending opinions on the subject of the *chara* of Cæsar.

M. de Berneaux, after having examined and rejected all these opinions successively, shows that the *chara* was a cabbage, and thinks it was the species now known by the name of *crambe tatarica*. In fact, this plant grows in great abundance in the environs of Dyrrachium, and in all Hungary and Turkey: it has very long and thick roots, firm and well tasted, which are eaten raw or boiled in all the countries just mentioned, and which are of great service in times of scarcity.

Several Latin authors describe various marsh plants by the name of *ulva*; but they mention one in particular as being excellent food for sheep. As among the aquatic plants the *festuca fluitans* only is eaten by sheep, and as this grass covers a great part of the Italian marshes, M. de Berneaux thinks the above is the particular species of *ulva*, and he also shows that this is the grass denominated by Theophrastus and the Greeks *typha.*

The ancients boast much of the useful properties of the *cytisus*; but they describe it imperfectly, and the moderns differ greatly as to the plant which ought to bear this name. Some have been of opinion that it is the tree lucern (*medicago arborea*). M. de Berneaux, who has made copious researches into this subject, thinks that it is rather our false ebony, *cytisus laburnum*. But as Pliny speaks clearly of this last tree under the name of *laburnum*, and regards it as different from the *cytisus*; and as on the other hand some parts of the description given of the *cytisus* by Dioscorides do not exactly suit it, M. de Berneaux's opinion does not seem quite well founded. It is to be observed, however, that Pliny and other ancient naturalists are very vague in their descriptions of plants.

In the buds of trees, there are some which are not developed with the rest, and which are called *dead eyes*, but which ought rather to be called *sleeping eyes*, for it is possible to revive them from this state of lethargy even when it has lasted several years. It is owing in general to the tendency of the sap to go to the upper buds. The lower buds are thereby deprived of this nourishing fluid. No inconvenience arises from this process to trees which are destined to yield wood or to afford a shade; but in the fruit-

fruit-trees, where it is requisite to arrange all the branches for a specific purpose, cultivators are compelled to put grafts on the places occupied by the dead eyes; a tedious and uncertain process. M. Marien de la Martinière practises another method: he makes a small cut in the form of a V reversed above the dead eye and into the core. The ascending sap is checked in its progress upwards, and is thus confined to the dead eyes.

We are now called upon to say a few words on a work by M. Lasteyrie du Saillant on all the branches of the agriculture and the rural and domestic œconomy of the Chinese. It is collected from all the authors who have written upon China, and embellished with a very great number of drawings made in China and by Chinese, in which are represented all the processes of their industry, and all the instruments which they employ. This great empire, in which an immense population is entirely supported by agriculture, and where this art has been honoured and protected without interruption since the first establishment of the Chinese monarchy, cannot fail to have made great progress; and in fact M. de Lasteyrie has made us acquainted with several utensils more simple and more convenient than those used by the Europeans, and indicates some highly useful improvements respecting the culture of fruit-trees.

M. Yvard has presented a large treatise on such plants as are injurious to grain, and on the means of preserving cultivated lands from them. What are commonly called bad plants are the production of nature; kinds of savage plants: the air, water, and animals bring their seeds, which the earth retains a long time in its bowels, and at the favourable moment they are seen to spring up: frequently also the farmer himself sows them mixed with badly compounded manure. M. Yvard, who has described upwards of three hundred, gives ample details of various methods for destroying these weeds.

This experienced agriculturist has rendered a still more important service to agriculture, by publishing last spring, through the medium of the journals, the means which his experience pointed out to him as the best adapted for repairing the losses which the events of the war had occasioned by destroying the corn in the ear. He had the happiness to see his counsel followed, and the high price of corn at least did not indicate that our provinces had been overrun by contending armies. It has been by similar applications of agriculture and of the arts, perfected by the spirit of the sciences, that France has combated for twenty years the disasters of a cruel *war*, and has been able to support, without sinking under it, the final operations which put an end to her miseries.

[To be continued.]

LVIII. *In-*

LVIII. *Intelligence and Miscellaneous Articles.*

SUTHERLAND COAL-PIT.

In the county of Sütherland in Scotland, a pit of coal was discovered about two or three years ago, contrary to the opinion of many who supposed that no coal was to be found north of the Tay. This coal has been wrought to a considerable extent, but time has shown that it seems to possess one property peculiar to itself. The refuse coal, of which a large quantity had been left to accumulate near the mouth of the pit, after having been exposed to the air for a considerable time, took fire of its own accord, and continued in a state of combustion till the whole was consumed. At present they have ceased to work the pit, partly on account of this peculiar property of the coal, but chiefly that they may have time to clear away the refuse on the surface. They do not despair of opening the pit again, and of discovering a mode of preventing the deflagration: and preparatory to the recommencement of working it, they are sinking shafts in the direction in which they intend to proceed.

We are sorry to announce the death of the celebrated Danish astronomer, Professor Bÿgge, of Copenhagen.

Mr. Thomas Forster has just published a Sketch of the New Anatomy and Physiology of the Brain. He has determined the organ of mysterizingness to be a distinct faculty, but does not enter into the proofs, as the large work of Dr. Spurzheim contains the substance of what is known on the subject.

LECTURES.

Theatre of Anatomy, Bartlett's Court, Holborn.—Lectures on Anatomy, Physiology, Pathology, and Surgery, by Mr. John Taunton, F.A.S., Member of the Royal College of Surgeons of London, Surgeon to the City and Finsbury Dispensaries, City of London Truss Society, &c.

In this Course of Lectures it is proposed to take a comprehensive view of the structure and œconomy of the living body, and to consider the causes, symptoms, nature, and *treatment of surgical diseases*, with the mode of performing the different surgical operations; forming a complete course of anatomical and physiological instruction for the medical or surgical student, the artist, the professional or private gentleman.

An

An ample field for professional edification will be afforded by the opportunity which pupils may have of attending the clinical and other practice of both the City and Finsbury Dispensaries.

The Summer Course will commence on Saturday, May 20, 1815, at Eight o'clock in the Evening precisely, and be continued every Tuesday, Thursday, and Saturday at the same hour.—Particulars may be had on applying to Mr. Taunton, 87, Hatton Garden.

Dr. Clutterbuck will begin his Summer Course of Lectures on the Theory and Practice of Physic, Materia Medica, and Chemistry, on Friday, June 2d, at Ten o'clock in the Morning, at his House, No. 1, in the Crescent, New Bridge Street, Blackfriars, where further particulars may be had.

The Summer Courses of Lectures on the Theory and Practice of Physic, by Dr. Roget; and of Materia Medica and Medical Jurisprudence, by Dr. Harrison, will commence, as usual, in Windmill Street, on the first week in May. The Lectures on Chemistry will, in consequence of Dr. Davy's absence from Town, be given during the Summer by Dr. Granville.

LIST OF PATENTS FOR NEW INVENTIONS.

To Charles Gent and Square Clarke, of Congleton, county palatine of Chester, for a new method of making a swift and other apparatus thereto belonging, for the purpose of winding silks.—21st March 1815.—2 months.

To Richard Smith, of Tibbington House, in the county of Stafford, ironmaster, for improvements in smelting iron stone or iron ore, lead or copper ore, and other mineral substances; also of refining crude iron, lead, copper, gold, silver, tin, and all other metals or metallic bodies, and of making and manufacturing iron.—29th March.—6 months.

To William Vaughan Palmer, of Ilminster, for an improved method of twisting and laying of hemp, flax, ropes, twine, line, thread, mohair, wool, cotton, silk, and metals, by machinery.—4th April.—2 months.

To Thomas Bagot, of Birmingham, for an improved method and machine for passing boats, barges, and other vessels from a higher to a lower level, and the contrary, without loss of water.—4th April.—3 months.

To William Losh, of the parish of Walls, Northumberland, iron-founder, for his new plan for fire-places or furnaces for heating ovens and boilers, and the water or other liquids contained

tained in boilers, and for converting such water or other liquids into steam for the purpose of working engines, and for other uses in manufacture.—8th April.—2 months.

To Joshua Shaw, of Mary Street, Fitzroy Square, for certain improvements in the tool or instrument called the glazier's diamond.—14th April.—6 months.

To William Bell, of Birmingham, for his new and improved method of making and manufacturing wire of every description. —18th April.—2 months.

To Michael Billingsley, of Bowling Ironworks, in the parish of Bradford, in the county of York, engineer, for certain improvements in the steam-engine.—20th April.—2 months.

METEOROLOGIAL RESULTS

Of the atmospherical Pressure, Temperature, Rain, Evaporation, and Wind; deduced from diurnal Observations made at Manchester in the Year 1814; by Mr. THOMAS HANSON, Surgeon.

Latitude 53° 25′ North. Longitude 2° 10′ West of London.

1814.	PRESSURE.					TEMPERATURE.				
	Mean.	Max.	Min.	Range.	Greatest Variation in 24 Hours.	Mean.	Max.	Min.	Range.	Greatest Variation in 24 Hours.
January, ...	29,516	30,50	28,73	2,50	,83	27°,70	41°	10°	31°	17°
February, ...	30,163	30,76	29,03	1,73	,47	36,30	50	22	28	17
March, ...	29,790	30,84	28,35	2,49	1,08	28,90	58	24	34	18
April, ...	29,883	30,50	28,86	1,16	,54	51,40	68	36	32	23
May, ...	30,149	30,85	29,30	1,55	,50	50,08	68	34	34	28
June, ...	30,121	30,48	29,78	,70	,20	55,50	75	39	36	28
July, ...	29,991	30,34	29,70	,64	,32	62,40	80	50	30	22
August, ...	30,001	30,48	29,66	,82	,36	59,37	71	46	25	16
September, ...	30,153	30,48	29,62	,86	,32	56,06	74	40	34	24
October, ...	29,833	30,46	29,12	1,34	,70	48,16	65	30	35	20
November, ...	29,777	30,22	29,28	,94	,59	41,01	53	23	30	15
December, ...	29,422	30,20	29,16	1,04	,64	39,79	56	27	29	20
Annual Mean,	29,899					47,22				

Conti-

Continued.

1814.	RAIN.			EVAP.	BAROM.		WIND.											
	Inches.	Wet Days.	Upon Blackstone Edge.	Inches.	Spaces.	Changes.	NORTH.	N. East.	EAST.	S. East.	SOUTH.	S. West.	WEST.	N. West.	Variable.	Calm.	Brisk.	Boisterous.
Jan. ..	,660	1	,700	,284	7,40	11	4	6	6	4	2	2	0	2	4	1	5	0
Feb. ..	1,255	8	2,690	1,100	4,80	6	0	4	2	5	3	7	4	2	1	0	4	0
March,	1,045	7	2,525	1,279	7,10	12	0	6	5	3	2	10	3	0	2	0	0	0
April .	2,660	18	4,160	2.042	4,85	7	1	0	0	9	7	10	1	2	0	0	3	1
May ..	,550	5	,640	2,770	5,50	13	0	9	8	0	3	6	2	0	0	0	4	0
June ..	1,875	13	1,460	2,530	2,40	9	0	4	3	4	3	8	5	2	1	0	3	3
July ..	2,735	15	2,300	2,797	3,50	11	0	0	0	2	2	19	3	3	2	0	3	0
August	3,700	22	4,720	2,098	2,65	9	0	0	0	0	1	11	12	7	0	0	2	1
Sept...	,970	6	1,700	2,116	2,05	5	0	1	2	0	3	12	2	7	3	0	2	0
Oct. ..	4,680	19	4,410	1,209	4,40	13	1	0	8	3	0	16	0	2	1	0	0	1
Nov...	2,055	19	3,430	,834	4,00	10	0	3	5	0	0	3	15	1	3	0	2	0
Dec...	4,305	13	5,250	1,104	4,60	17	0	0	5	3	7	7	0	5	4	0	5	1
Total	26,470	146	34,985	20,163	53,25	123	6	33	44	33	33	111	47	33	21	1	33	7

January.—A low state of temperature prevailed nearly throughout this month, the daily means fluctuating between 20° and 30°, and in one instance (on the 17th) the minimum was 17°; this low state of temperature was preceded by heavy falls of snow, accompanied with gentle easterly winds.—Very little rain fell, as might be expected from the above occurrences; my register exhibits only one day on which rain fell, which was on the 26th, at the time of a general thaw:—The thermometer soon indicated 41°.—Snow fell at intervals on 13 days, and in four instances was attended with hail.

February.—This month was considerably more mild and humid than the preceding, yet in a few instances it was attended with low nightly temperatures, which were, in the course of a few days, considerably augmented, so as to make the daily means average between 30° and 40°.—On the 4th, the temperature was as low as 22°, but on the 12th at 50°.—Monthly mean, nearly 10° more than that of January.—Four days were marked by falls of snow and hail.

March.—The first twenty days were cloudy and cold, attended with frequent showers of snow: in one instance, viz. on the 5th, the snow assumed a very singular and regular arrangement of its particles, which were in the form of small flat semi-incrusted *stars*, each being formed of *six radii*. All that I saw had the same number of radii, and the stars were nearly all of a size.—On the 9th, snow fell in larger quantities, having the same appearances as on the 5th.—Towards the latter part of the month, the weather became very mild, for the temperature frequently rose to 58°, attended with a gentle SW wind, which, with a NE wind, have been the most prevailing ones.—This period has been free from either strong or boisterous winds.

April.—The temperature and fall of rain were mild and congenial to vegetation to the 14th; the atmosphere was also brilliant.—After this period, clouds soon gathered, and rain fell at intervals to the end.—On the forenoon of the 15th there was a sudden gust of wind.—On the 17th, a boisterous SE wind.—On the 19th rain fell almost incessantly the whole of the day.

May—Was colder than the preceding month, although the atmosphere was generally clear, and attended with very little rain: the reason was a in great measure owing to the prevalence of the east and north-east winds.—On the 8th there was a shower of snow.

June

June.—The first fortnight was cold and cloudy for the season, particularly from the 1st to the 9th, as the wind chiefly blew from the east quarter, which had the effect of retarding vegetation, particularly as the fall of rain had been trifling for six weeks past.—It is remarkable, that during this and the preceding month the greatest extremes took place in the course of twenty-four hours: in the present instance the difference of the extremes of the thermometer indicated 28°, the maximum being at 67°, and the minimum 39°: it was on the 9th, or upon the day these changes took place, that the wind changed from E to S.—Much honey-dew appeared upon currant-trees during the continuance of the easterly winds. The latter part of the month was warm, and attended with occasional showers of rain, which much revived the drooping state of vegetation.

July.—Rain fell at intervals, and in larger quantities; which greatly accelerated the productions of the ground.—On the 20th, about four o'clock in the evening, there were very vivid lightning and loud thunder; after which an inch and a half of rain fell in the course of half an hour; wind blew strong but sultry from the SW. previous to the storm, but about the time of the thunder it became a complete calm.—The temperature in consequence was lowered, but it soon showed a quick and great augmentation, for on the 25th it was as high as 80°, being the annual maximum.

August.—This month was decidedly very cloudy and showery, for there were 22 days on which rain fell less or more, yet the monthly quantity only exceeds the preceding month by one inch; the temperature in consequence received a sensible diminution.—Prevailing winds, SW and NW; on the 6th and two following days it blew strong and boisterous from the former quarter, attended with heavy rain.

September.—From the 1st to the 12th, the curve of temperature was lowered 16°, the minimum being as low as 40°, but at the end of a week from this depression it was raised to 74°, which constitutes the monthly extremes.—This month has been fine, brilliant, and dry; prevailing wind SW.—On the 11th, at nine o'clock in the evening, the sky being free from clouds, there was a luminous band, like an halo, but much narrower, which extended quite across the hemisphere in the direction from NNE to WW by S; it continued near an hour, gradually disappearing, by diffusing its light laterally.

October.—From the 1st to the 12th the weather became much colder, the thermometer indicated two degrees below freezing, but which was at the end of four days raised 26°; wind boisterous from SW; rain now fell in large quantities, and continued so almost uninterruptedly to the end.

November—Was noted for fluctuations of heat, interspersed with gloomy and rainy days.—Mean monthly temperature about 40°, the minimum was as low as 23°; and which occurred on the 22d.

December—Upon the whole was as mild as November, indeed the two monthly extremes were several degrees higher.—On the 16th the wind blew a violent hurricane from the SSW nearly the whole of the day, its fury did much damage to houses and other buildings by unroofing and otherwise damaging them.—There was a great and sudden loss of barometrical pressure, for in the course of the day the mercury lost nine-tenths of an inch.—Early on the morning of the 14th there were thunder and lightning.

The annual pressure of the barometer is	29,899 inches
Ditto of the thermometer	47°,223
Mean temperature of the six summer months	58°,80
Ditto ditto of the six winter months	38°,64
Fall of rain in Manchester	26,470 inches
Ditto upon Blackstone Edge	34,985 do
Water evaporated from a surface of water	20,163 do
Spaces described by the barometer from its mean daily curve	53,25 do
Number of barometrical changes	123 do

Prevailing winds, SW. W. and E.

Meteoro-

Meteorological Observations made at Clapton, in Hackney, from April 1 to 20, 1815.

April 1 to 7*.—The weather during this period was fine, warm, and dry; the wind for the most part westerly, with clouds of curious modifications. Some slight showers now and then.

April 8.—I perceived today that the wind was got to the eastward. There were *cirri* and a cirrocumulative tendency in the clouds, with *cumuli* below. The wind blew strong.

April 9.—Easterly wind. Cloudy, with a shower in the middle of the day.

April 10.—Hard rain early; clouded and damp the rest of the day. Thermometer at 11 P.M. 52°. Barometer falling 30·00. Wind easterly.

April 11.—Cloudy morning and misty; fair day, and very warm. Fine calm evening. *Cumuli* and some *cirrostratus*. Thermometer at midnight 45°. It had been 62° in the day.

April 12.—Fine warm day, but with light showers.

April 13.—A very hard thunderstorm with hail and rain about 2 P.M.; air warm; night partially clouded. Some of the hailstones which fell during the hard storm this morning were very large. The evening became cool.

April 14.—Cold north wind. The change from the warmth of the several preceding days to the present cold was very sensibly felt.

April 15.—Cold N. wind with much cloudiness. *Cumulosratus* forming out of the flimsy and confused modifications. The Barometer rising to 30·20 at 11 P.M., and the Thermometer down to the freezing point.

April 16.—Confused masses of lofty *cumulus* early; afterwards common ephemeral well-defined *cumuli*. Cold north winds.

April 17.—Fine clear morning; *cumuli* formed afterwards.

April 18.—Cold NE wind, and unpleasantly dusty. Fine clear moon-light evening.

April 19.—The weather still cold; the sky was clear, except *cumuli* increasing towards afternoon. The moon at night was hazy with a faint *halo*, and the air was somewhat warmer.

April 20.—Clouded and warmer than yesterday, with change of wind, veering round to the eastward. During the morning some small rain. Barometer sinking.

Five Houses, Clapton,
April 22, 1815.

THOMAS FORSTER.

* A brilliant meteor was seen at Walthamstow about 11 o'clock in the evening of the 2d. It had an ascending and curvilinear motion.

METEORO-

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand,

For April 1815.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
March 27	50	55	55	29·52	25	Stormy
28	55	56	49	·75	32	Fair
29	50	60	50	·98	40	Fair
30	47	55	49	·99	34	Cloudy
31	51	48	51	·91	55	Fair
April 1	53	68	56	·68	78	Fair
2	56	61	50	·88	60	Fair
3	47	56	45	·90	57	Fair
4	45	58	44	·92	62	Fair
5	44	58	44	30·28	60	Fair
6	45	57	55	·20	45	Fair
7	47	67	50	·11	47	Fair
8	50	60	49	29·98	51	Fair
9	49	60	40	·96	40	Fair
10	49	58	52	·96	0	Rain
11	50	62	54	·94	61	Fair
12	52	63	50	·92	54	Cloudy
13	50	56	44	·68	0	Thunder and Rain
14	40	45	39	.72	40	Sleet and Rain
15	38	44	40	30·13	37	Cloudy
16	40	45	39	·15	50	Fair
17	39	52	41	·28	46	Fair
18	42	53	42	·38	48	Fair
19	42	53	41	·16	49	Fair
20	45	55	41	29·82	41	Cloudy
21	42	43	40	28·85	0	Rain
22	44	47	43	·93	0	Rain
23	43	55	42	29·20	0	Stormy
24	42	46	42	·42	0	Stormy
25	43	45	44	·70	24	Cloudy
26	45	54	43	30·05	25	Fair

N.B. The Barometer's height is taken at one o'clock.

LIX. *On the Phænomena of Vegetation.* *By Mrs.* Ibbetson.

To Mr. Tilloch.

Sir,—In two previous communications I have shown that the quantity of nourishment distributed to plants was variously arranged between the two grand repositories, the earth and the atmosphere. I have by the dissection of leaves, plainly I hope, evinced that some plants receive hardly any nutriment from the soil, while from the surface and sides of the leaves, and the immense quantity of hairs with which the plants are loaded, they evidently prove how much support they draw from the atmosphere. But others on the contrary, having innumerable skins on their leaves to shut out moisture, must therefore depend almost wholly on the root for that support which they cannot procure otherwise. Now to complete the establishment of this fact, as one to be depended on, it was necessary that the formation and appearance of the root of each different plant should corroborate the evidence of the leaves, and answer to the idea their cuticles suggested. I had hitherto neglected to examine this; I had indeed dissected a great variety of roots, but it was the interior that then occupied my attention, not the exterior and accompaniments. It was with some trepidation, therefore, I collected twenty roots of different soils to examine and inquire thoroughly into this fact. With what delight then do I present to the public the complete confirmation of its truth! and as this requires no dissection, every person may prove the justice of my assertion. But before I show the sort of root that belongs to the plant of each different soil, I shall point out the various ways in which the juices are received by the root, and the use of each separate part.

The middle root is merely a reservoir, within which is accumulated all that provision which supports the plant. Its office appears to be the receiving the collections brought to it by the side roots, to arrange, compound, and extract the juices collected for its appropriation and selection. Next to this are the side roots:—always keeping near the surface of the earth, they *cull* the *richest* of its liquids, and from the fat and vegetable matter which usually covers the upper soil, they by means of their radicula, which are generally projected upwards, draw into these side roots that liquid matter which forms the sap, but which is certainly *diluted* in the middle root; for, if caught in the preceding situation, it forms an almost *jelly;* yet when arrived at the centre it is perfectly *liquefied.* Thus these roots bring the nutriment requisite for the tree, and the matter which

forms the new wood, as I have before shown in Nicholson's Journal, vol. xxxiii. p. 234. The tap-root collects the juices from a lower strata from the subsoil; and *there*, I doubt not, much of that which completes the bark is taken, besides the matter of the pollen, which is most evidently formed here, since I have traced it from hence to the parts adjoining the bark, and from that place to the bud at the time it is taking in its seeds and pollen, when fixed in its cradle in the bark. The juices thus obtained from the substrata, and running in the tap-root, often show by their strange colours the different kinds of liquids they draw from the earth: from the side roots no juices come but an inspissated colourless liquid, but in the tap-root it is often so deeply tainted with colour as to dye the wood the whole way it runs. I have by me three specimens of this:—one a beautiful lead tint, one a bright yellow, and one a light green: the latter I take to be *copper*, especially as it killed the tree. The yellow was probably sulphur; but the first had the appearance of a bright and shining paint, highly *varnished*. After the tap-root may be ranged the radicula: and this is the principal part that really draws in the nourishment the plant requires; and by their number we may fairly judge what quantity of matter the tree takes in from the root: they are formed in a curious manner, rounded at the end and without rind, and more intended apparently to suck up the moisture, than take it in as the hairs. I may here observe, that when it is designed to take in juices from the atmosphere, the instrument made use of by Nature resembles a blow-pipe, though with many valves. But when the nourishment is to be drawn from the earth, the instrument is a round figure like a diminutive sponge about 2-10ths of an inch in length, but which has also several valves to complete it; see Plate VII. fig. 1. Besides the radicula there is another sort of small root, which I call a *fibre:* it is blunt, with an interior vessel often projecting beyond the bark: see fig. 2. Next to this are the hairs, which are rarely discovered of more than two sorts; see fig. 3. But the hairs appear to me to be merely an occasional addition, when a dry season requires more moisture.—I have now shown how the root receives the liquids from the earth; it may therefore be easily conceived that the quantity of matter it takes in, must be proportioned to the *number* of *radicula*, *fibres* and *hairs*, it possesses, and not to the *size* of its middle *root*.

This prefaced, I shall now show that the sand plants, which take so much nutriment from the leaves, though often large in the middle (as the turnip), have a totally *denuded* root, with very few radicles and no fibres; they have long tapering roots lessening by degrees to a point; they have a few side roots rarely graced with radicula. See the turnip-radish, carrot, chamomile, *urtica*

urtica urens, *alyssum*, and many others it would be tedious to mention, but which take almost all their nourishment from the atmosphere*; see Plate VII. fig. 4.

Rock plants being of a perfectly different form, have no *radicula;* they have instead a little sort of instrument which licks up the water from the stones, to keep the stem moist enough to allow the seeds formed in the root to rise through it to the buds. The trifling degree of earth found in most rocks appears to give sufficient liquid to form the seeds, and this is all that is required from the root of a rock vegetable; for all the nutriment is given either by the leaves if they have any, or the stems and flowers of these plants: every part is indeed open to the atmosphere. But when we turn from the vegetable, which, like the *rock plant*, takes all its nourishment from above, and observe those which by degrees receive more and more of their support from the root, we are immediately struck with the difference not only in *form* but in *appendages.* The clayey plant receives a pretty equal quantity of nutriment from the *leaves* and *root*. It has almost always a stunted *præmorse* or bitten-off root; see fig. 7. The side roots reach but a very little way, that they may not be *cut off* by the large lumps of clay that coagulate, and inclose the moisture surrounding each vegetable. But their short side roots are loaded with radicula, which plainly show how much more nutriment they must take from the earth than either *sand* or *rock plants:* sometimes they have occasional hairs, but never any fibres.

The chalk plant takes nearly two-thirds from the root, the rest from the atmosphere. They have no very distinguished shaped root, that part being sometimes large and thick, as in *Japonaria officinalis* and *Artemisia acantha;* sometimes running, as in the *Antirrhinum repens:* they have always pretty nearly the same quantity of *radicles*, many more than the clayey plants, but no fibres, and rarely any hairs. Most of the chalk plants have rather a thick cuticle to their root, but not so thick as that which always covers the clayey plant, for that is almost always double: both are intended, I suppose, to guard the root from the entrance of that putrid water but too apt to lodge within the interstices of the *clayey lumps*. I conjecture this, because the *defence* (though still existing) is not so great in the *chalk* as in the *clay*, neither is the first so *troubled* with the *defect*. See fig. 8, *Chalk root*.

* I much regret space is not allowed me to give many specimens in drawing of the roots, as they would strike the eye with the astonishing difference existing in *that part* as appertaining to each different soil. But should I ever be able to give this work to the public properly, it will be there exemplified.

 As

As to the tree and shrub, they are certainly more indebted to the root than to the atmosphere, though a quantity of nutriment is also received by the leaves. It is known that in many trees the roots almost equal in extent the spreading branches; and as each of the side limbs below has such a set of branches to support above, they must of course increase with their leafy honours. It is the side roots which bring the greatest quantity of sap: they often reach an immense way; and as the radicles placed in the whole extent are mostly turned upwards, it is in great part the produce of that vegetable matter lying near the surface which bestows on them its richest juices. A large number of circular vessels in the middle of these roots convey these treasures to the centre root, while the *rest* (adjoining the bark) are filled with fresh-forming *seeds*. Trees and shrubs as well as luscious plants (see fig. 9) have all the assistance the radicula, as well as fibres and hairs, can bestow on them; but many have also those balls of fat matter which adhere often to the sides of the fibres, and must add much to the nutriment of the plant. But when it is a fir or an ever-green tree, Nature has still another resource. Dissecting many roots of firs, I discovered in the balm of Gilead fir, at the termination of each side root, a large bundle of radicula; it had a most strange appearance; nor could I at first understand its use, till I examined it thoroughly, and found it was to increase the nourishment within a given distance. To give an idea of the contrivance, I subjoin the specimen; it is however scarce one quarter of it, see fig. 5. Besides this, the tree had fibres, hairs, and *balls;* it must therefore have had bestowed on it ample amends for the nutriment it lost in its leaves. In the rich loams, and those plants which are only suited to them, the quantity of roots is prodigious; I have often seen them twice the size of the plant, for example the *rape plant*, see fig. 9, which draws much juice from the earth. The bog plants also have many spreading roots, and are extremely indebted to the soil for support.

But I must not pass over the curious form of the roots of the water plants. They hang like a syphon in a straight line from the middle root, making first a sort of curve like that instrument, see fig. 6: by this means, and by a managed vacuum above, they draw up the water to the plant, as the impervious skin *round the root* will not permit it to enter in any *other way*, though growing *in* or *on* the water: the stem of the leaves serves to communicate the air at the first return of light; and when this is drawn off by the wants of the plant, it completes the *vacuum*, which draws up the water in the *hanging roots*, which had been *closed* by *cold* and *darkness*, and re-opened by *light* and *air*. If the roots are taken out of the water at night, they will always be found

found closed and their sides pressed together. There is still, however, much to be discovered in the mechanism of the water plants, which I hope to understand and unravel this summer: the straight roots of the real water plants are never branched, nor have they any radicula, fibres, and rarely any hairs, and those only on the middle root. I before showed that the leaves take in no nourishment from the atmosphere; but these pipes hanging continually in water, and administering it to the plant as long as the light continues, must give all the support they can possibly require.

Thus it may be easily seen that the roots as well as the *cuticles* of *the leaves and the hairs* confirm the facts they promised to corroborate, and prove that the plants are either fed by the roots or the atmosphere, or both conjointly; that this entirely depends on the nature of the soil. What then is to be deduced from this review of their habits? That each plant being formed to grow in one particular earth, it must consequently be more adapted and *grow better* in its native soil than in any other. That those plants which grow in *sand*, from taking so much from the atmosphere, require a greater attention to be paid to the aspect *than any* other *circumstance.* That those which grow in clayey ground demand its being drained, and reduced to as fine a soil as is consistent with the nature of the earth; that the roots may be capable of running to a greater distance from their centre root, *without danger* of having them *cut off*, and therefore that they may, *by spreading more*, be better able to nourish the plant. In calcareous ground the form of the root teaches us that the weight of the chalk should be lessened by mixture, and the secreted water mingled with the upper surface: and in a rich soil it should be well ascertained, whether it is luscious enough to do without manure; as all these luxuriant plants are so loaded with the means of taking in nutriment from the root, that if any addition is made they will easily overpower themselves, and die of plethory. It often happens that dung is laid on land that would grow better for a few cart-loads of clay or sand. I have seen a soil so rich as to spoil the taste of the vegetables raised in it, and to bring on disorders that afterwards spread in the seed. It is greatly the interest of the farmer who is lucky enough to have such land, to ascertain this, as it is only not to manure it, and now and then to throw on it a little lime to prevent the acid too strongly predominating over the alkali, and the plants cannot fail to do well. It is astonishing how much better the East Indians understand this subject than we do: they would not put a plant in a wrong soil *for the world.* They say very properly that it will *blight it*, and they have always proved it; for when Tippoo Saib insisted on their putting in

a particular sort of wheat (*Triticum monococcum*) in the Mysore country, which was *not suited to the soil*, though it did admirably higher up towards the Carnatic—obliged to obey, all the wheat that was produced was so tainted with blight, that it yielded in a bushel not a quarter measure of flour. When for want of other ground they are obliged to put a plant into clay instead of sand, they fill the holes where the seeds are to be placed with sand, or strew the ground over with it to some depth*, that the embryo of the plant, finding *its own soil*, may draw from it its *native juices*, nor have to struggle with adverse land, till, strong and vigorous, it is enabled to bear it. But they say, "take what precautions they will, it is never equal to that which grows in its *proper soil*." I think I may observe, that it stands to reason that this must be the case; and that, as every plant has its favourite earth, it must grow better and finer, if manured *in that soil*, than *in any other*: and as Nature has been so bountiful to us as to bestow a great variety of the necessary plants, and adapted them to almost every soil; should not our industry *supply* what remains to be done, and seek and fix the plants suiting each different sort of ground? And would not the person who did *this* be a *benefactor* to *mankind?* The manner in which our wheat is now tainted, calls aloud for some remedy; and the introduction of the immense number of *new wheats* well accounts for the increase of this disorder; for they are placed in any ground, nor the question thought of "From what soil do they come?" But I have strayed to farming again, when I *intended* to *pursue* my *present subject*.—"The consequence that must follow the proofs I have given of the leaves in part feeding the plants."

If the leaves contribute thus to the nutriment of the plant, and that the water taken in by the hairs is decomposed, and converted into oxygen and hydrogen, the first given out to purify the air, and the latter secreted for the use of the seeds; how can it be also retained to be given out in perspiration? The plan is perfectly contradictory; and I wonder not that the *inimitable Mirbel* expressed some doubts of its truth. The idea therefore of plants perspiring *is a mistake;* and those figures taken for *bubbles of water* given out of the plant, are on the contrary instruments beautifully adapted to the *receiving* and *inhaling* all the various juices the atmosphere has to bestow, and convert them into *volatile oils*, *resins*, and all the liquids the plant is afterwards found to yield. But another argument appears to me still more to show the fallacy of the former opinions. It was believed that the water *taken in* from the dews and rains

* See Buchannan.

was

was *decomposed*, and the oxygen given out; and the only remaining juices, the sap, (after mounting and attaining the highest part of the tree,) *descended again* through the bark into the root. Thus every sort of nutriment belonging to the plant is disposed of. And what then is left to nourish the plant? what is to form all the new shoots of the tree, and new wood which requires the sap to stagnate on the parts while forming? But, instead of this false system, if we suppose *that part* only *just* which *dissection authorizes* and enforces, "the dispersion of oxygen arising from decomposed water," and the very pockets from which it flows, is discoverable in the leaves: that from other sorts of hairs are taken into the plant all that can form the scents and sweet-smelling oils, and are thus made to pass into the second cuticle, where they are defended by a double skin from the too great evaporation; while the various bark juices after uniting compose the pabulum of the leaf; then pouring down through the leaf-stem, pass under the rind, and fill the large vessels of the inner bark, the green part detaching itself and forming separate as in the leaves; while the upper cuticle of the leaf gives nutriment to many plants, and with the assistance of the hairs and instruments gives scents or juices to all: that through the root and from the earth is taken the sap, which is always analogous in quantity to the measure of the new shoots it is to form, and the new wood it is to create: that the tap-root also supplies the centre shoot and the powder of the pollen. Thus every part is provided for; and the whole formation is justified, and indeed was at first suggested by the dissection of plants, and can in its whole process be followed up by the eye, with the help of little more than a single microscope, though the discoveries of course were made with *much more powerful means*. It is also certain that the roots mark, by their *shape* and their *accompaniments*, the soil in which they are to grow; it cannot therefore be wondered at, that the cuticle of the leaves of each plant should concur with them to display the same.

It was my intention to leave the root of rock plants undescribed for the present, till I got some more vegetables of that kind; but I am anxious to bring them all into *one point of view*, and show how entirely the soil governs the plant, and how truly the root differs in *shape*, *formation*, and *accompaniments*, when it originally proceeds from another earth; and in this case there are very strange alterations I cannot in any manner account for. The stem from the root upwards is exposed and open to the atmosphere, and generally covered with hairs or *spines*, which is only another means of taking in nourishment; but the root below is laid over with a double cuticle of a very strange kind.

It is uncommonly *thick*, and the whole interior of the stem *so loose* as not to be half filled, and the centre circle is a cylinder of cotton curiously formed: this I take (though so soft) to be the *wood part*, as there is an interior line, *small* (but more solid in matter), on which all the branches of *the root* are *fixed*, which proves it to be the line of life, or *canal médullaire*. The alburnum vessels, instead of being in *their usual places* (between the bark and wood), are placed between the two rinds; and the inner bark vessels follow. We may call the second rind a bark, for there is *no other:* but if we do, it is still differently placed, since it completely *separates* the *seeds* and *inner bark vessels:* the pollen is as usual with the seeds, only the latter in *vessels tied* with the string, and the farina in the spaces between them; the two rinds are more like *leather*. The wood (if it must be so called) consists of soft cotton cylinders filled with water, which I suppose is taken up by the little pumps which are on the *roots* instead of radicles: but there are *two sorts;* perhaps one of them takes the juices for *the seeds*, and the other *the water*, as there are very few of *the former;* see fig. X, and fig. XI; the water ones, fig. XII, the shape of the wood vessels. The seeds are most easily seen mounting the tree from the root in the *stone pine*, and red *spruce fir*, as also in the *sempervivums*, if the two rinds are divided.

I shall now conclude this letter, and my next will be another explanatory letter on the growth of the seeds in the root, showing that they must be *seeds*, or at least *the embryo* or *heart;* since they are to be seen passing into the seed-vessel and fixing themselves in the place of the seeds while the flowers are still in the bud.

I am, sir,

Your obliged servant,

Sherwood, Jan. 29, 1815. AGNES IBBETSON.

A further Description of the Plate.

Pl. VII. Fig. 1. Appearance of the radicles when dissected.

Fig. 2. Form of the fibres only to be found in rich ground, trees and firs.

Fig. 3. The only sort of hairs found in the root.

Fig. 4, 4. The sort of roots common to all *sand plants*.

Fig. 5, 5. The quantity of radicles found in the root of the silver fir.

Fig. 6. The sort of root common to all *water plants*.

Fig. 7. Root common to *clayey plants*.

Fig. 8. Roots common to many *chalk plants*.

Fig.

Fig. 9. Root common to all rich earth: the root being larger still.

Fig. X. The first sort of pump; the second sort of pump, fig. XI.

LX. *A Reply to Mr.* Donovan's *Observations, &c. on Mr.* De Luc's *Paper published in our Number for February. By* J. A. De Luc, *Esq. F.R.S. &c.*

To Mr. Tilloch.

Sir,—In your Journal for March, art. xxxvii. I find "Observations on a Paper by J. A. De Luc, Esq. containing some Remarks on Mr. Donovan's Reflections concerning the Inadequacy of electrical Hypotheses; by M. Donovan, Esq."

If philosophical controversies were always carried on with such a tranquillity and fairness as reigns in that between Mr. Donovan and myself, such discussions would better forward true natural philosophy than they commonly do. I hope it will be the case when I shall have explained myself respecting what appears a disagreement between us.

A first point relates to the object of *excitation.* I had not present to my memory all the parts of Franklin's theory with regard to that part of electric science, when finding it in Mr. Donovan's paper, and considering it as his own opinion, I made some objections against it; therefore he has reason to say *that it does not prove against him, but against Franklin;* in proof of which he quotes the Doctor's work, "Experiments and Observations on Electricity," published in London in 1749.

The most important point in our controversy concerns Volta's theory, of which I thought to give a proof by an experiment against which Mr. Donovan objects. The theory of that justly celebrated electrician is, that the *standard* of *plus* and *minus* in our observations is no *fixed point,* but *changeable,* being the *actual electric state of the ambient air.* My experiments to prove it, which I still consider as decisive, Mr. Donovan accurately describes, and I shall repeat them in his own words; in which, however, he uses an expression which has misled him, and which I shall point out afterwards.

"By the continual dispersion of electricity in a room (where an electric machine was worked, having a *point* fixed to the prime conductor), the *air* of the room was rendered *positive*; a pair of insulated pith-balls in the *natural state* was brought in from an adjoining room; they were diverged *negatively;* but when they returned to the room whence they came, the *natural state* was

was resumed. Mr. De Luc explains this by supposing, that although the balls were at first in the *natural state* when compared with the electric state of that room; yet, when brought into an *air* containing a greater absolute quantity of electricity, they became relatively *negative*. Now if this admits a different explanation, the necessity of the above inference is destroyed; it is therefore of importance to try if this can be effected..... Now it appears to me, that the principle of *electric influence* may be applied in explanation of Mr. De Luc's experiments on electrified air. The balls in the *natural state* are brought into a *positive atmosphere;* the electricity of the latter *repels* the natural quantity of the balls into their *internal substance;* the *external parts* therefore are left *minus*. With this view the sequel exactly corresponds; for, when the balls were brought back into the *unelectrified room*, they collapsed into their *natural state*. But on the opposite view, the balls by immersion into a *positive atmosphere* would assume that state and retain it when removed, and therefore continue to *diverge*. Hence it appears to me, that the experiment is an anomaly on the hypothesis of Volta, and perfectly reconcileable to the principle of Franklin."

I think that some further explanations will remove Mr. Donovan's difficulties; and the most direct way will be by stating in what consists the difference between the two theories.—In Dr. Franklin's, the *standard* of *plus* and *minus* was a certain (supposed) *natural quantity* of *electrity* belonging to all the substances of our globe, which fixation as merely arbitrary, and opposed by facts, prevented that theory, though true in itself, from being admitted by a great number of natural philosophers both in England and on the continent; it was even by such strong arguments explained in my works, that they were never answered by retaining that theory.

But Volta's theory has produced two great and permanent changes in the science of electricity. He first proved that there is no fixed nor permanent *standard* of *plus* and *minus;* that the *standard* was *changeable*, being the *actual electric state of ambient air*. He next explained the cause of the *electric motions* of *pairs of balls*, by proving that *air* possesses the *electric fluid* as well as all the bodies which it embraces; but with this circumstance, that it abandons some to the bodies that have *less*, and takes some from those which have *more* than itself: lastly, that as the *electric fluid* has some *adhesion* to the bodies which possess it, if these bodies oppose less resistance to *move* than to part with the *excess*, or to receive their *defect*, they are transported by the *electric fluid* where it has a tendency to move.

I have demonstrated that effect by an analogous experiment made with a pair of small *soap disks* suspended by thin threads, as

as a pair of *pith-balls.* When these *disks* were laid together on *water* they *diverged,* and the cause of that *divergence* was *visible:* both *disks* gave *soap* to the *water* between them, but only *one* to the external water on both sides; and the *disks* moving towards this, they *separated* from each other, or *diverged.*

Returning to *electric motions,* Volta's theory was submitted to direct verification by the experiment which Mr. Donovan examines. But I shall first remark, that by changing the expression which I had used in explaining these phænomena, he has not undertood me, nor could he understand Volta's theory. He uses the expression *natural state,* to define the *electric state* of the *air* in the room next to that in which an electric machine was set in motion; whereas I had used the expression, the *actual electric state of the air in that room,* in which the *pair of balls* which were in the *same electric state* did not, nor could not *diverge.* If therefore Mr. Donovan in his reasoning changes the expression *natural state,* into that of *actual and local electric state of the ambient air,* he will find that his objection is not applicable to the explanation of the phænomena which I have observed.

Mr. Donovan makes this objection, "that the *pair of balls* brought from the next room, when coming in a *positive atmosphere* should be rendered *positive,* and continue to *diverge so,* when brought back to the first room." This was not the case, however. Therefore Mr. Donovan's objection is against a fact. But this would certainly have happened, had that pair of balls remained long enough in the room of the electric machine; for the *air* communicating at least its own *electric state,* they would then cease to *diverge;* but in that case, when brought back to the first room, instead of collapsing, they would have diverged as *positive;* having thus changed their *electric state.* Now Volta's *theory* explains all these cases: it is founded on this property of *air,* that it is a *non-conductor,* and cannot communicate the *electric fluid* to the bodies which it surrounds, or take some from them but in *absolute contact;* and from this circumstance proceeds the insulation of electrified bodies in *air.* However, by the continuance of the *contact* of the particles of *air,* these bodies are by degrees reduced gradually to its *electric state.*

With respect to *electric influences,* no true conclusion can be derived from experiments on that object when the *air* is not *dry.* For *moisture* is produced in the *air* by *aqueous vapour,* a *conducting fluid;* wherefore, in all my electric experiments, I had in the same room the *hygrometer* of my construction, and I made them at times when that *hygrometer* stood at about the same degree, as I have stated it in my paper in Nicholson's Philosophical Journal. Mr. Donovan had not this test in his experiments

ments with an *excited glass tube*, which he brought under a pith-ball, and when removing the *tube*, the ball remained *positive*. This effect was probably owing to the *aqueous vapour* being then abundant in the room, and thus producing a *conducting medium*.

Coming now to the great object of electricity, namely, the *divergence* of the *electrified ball*, which is our *electrometer*; Mr. D. declares with ingenuousness that he has not had opportunity to see Volta's system completely stated; but he says, if I understand, it relates to two bodies, one of which is in a *natural state*, and the other in a *super-natural state*. But this is a misconception: Volta's theory relates to two bodies, *both* either *positive* or *negative*; and this *divergence* I have thus explained from Volta's theory, as in a pair of balls thus situated, *both* act to bring the *air between them* to their *electric state*; while *one* only acts on the *external air* on *both sides*, to which therefore they tend decidedly.

Mr. Donovan objects against that explanation, "that it virtually destroys the principle of *repulsion*, and refers all to *attraction*." This is true in the common acceptation of the words *attraction* and *repulsion*; but in strict natural philosophy, they ought to be called *tendencies*, as a *visible effect*, not implying the idea of *cause*, of which we might remain ignorant. But Mr. Donovan asks, "How can it be supposed at the same time, that *electricity* is an *elastic fluid?*" In Volta's theory *electricity* is only considered as an *elastic fluid*, in the upper region of the atmosphere; for, when it is produced in a manner which I have explained, it darts in a straight line and soon vanishes, as seen in *lightning*. But within the atmosphere it is a parasite fluid, always fixed to the particles of *air*; and I have proved, by an experiment related in my work *Idées sur la Météorologie*, that a *perfect vacuum* free from *aqueous vapour* is not a *conductor*. This experiment I made in presence of Dr. Franklin and some other experimental philosophers, who, knowing my opinion, desired to submit it to a test which Dr. Franklin himself prescribed. I performed the experiment, by which it was completely proved that a *perfect vacuum* did not transmit the *electric fluid*. Thus, after having doubted, they were fully convinced of this fact, that the *electric fluid* is really a *parasite fluid*, always attached to the particles of some *atmospheric fluid* and moving with them.

There remains another object of disagreement between Mr. Donovan and myself, concerning the *impermeability* of *glass* to the *electric matter*, which in my works on *Electricity* I had maintained as the real cause of the phænomena of the *Leyden vial*. On this object Mr. Donovan refers me to an interesting experiment which he has made, and thus describes: "A thin flask

flask of glass with a neck many inches long was half-filled with mercury, and coated on the outside to the same height with foil. By means of a moveable wire, an electric charge was thrown in; the wire was drawn out, and the neck was *hermetically sealed*. After a length of time the sealing of the neck was cut off; the wire was plunged into the mercury, but not the smallest *commotion* was perceived by the hand; although the original charge was capable of giving a *violent shock*. Thus I had encompassed a quantity of electricity on all sides by *glass*: after a *certain time* I found that the *glass* contained none. What should I conclude, but that it *escaped?* Were Mr. De Luc to repeat this experiment, he would certainly consider my inference as natural."

I am too old and too infirm to undertake any new experiments; I shall therefore only suggest an idea which Mr. Donovan may easily try. *Glass* becomes a *conductor* when *heated*; thus it may be that the *flask* was *discharged*, by the operation of *sealing it*; for the operation of *hermetically* sealing it, requires a great *heat*. I therefore suspect that the moment when the *flask* was *sealed*, it was *discharged*. This appears to me more natural than to suppose, against all the known phænomena of the *Leyden vial*, that *glass* is *permeable* to the *electric matter*. But Mr. Donovan might easily put it to the test of experiment in the manner above pointed out.

I am, sir,

Your most obedent servant,

J. A. De Luc.

LXI. *Observations on the Priority of Mr.* Smith's *Investigations of the Strata of England; on the very unhandsome Conduct of certain Persons in detracting from his Merit therein; and the Endeavours of others to supplant him in the Sale of his Maps;—with a Reply to Mr.* W. H. Gilby's *Letter in the last Number. By Mr.* John Farey, *Sen.*

To Mr. Tilloch.

Sir,—The rash and unfounded reflections, which a Mr. *W. H. Gilby* has been induced to address to you from Edinburgh, pages 300 and 301 of your last Number, would not at this time have occasioned me to trouble you thereon, but for the very superior calls of *justice* as well as friendship, in favour of Mr. *William Smith's* irresistible claim *to the discovery of the order*, and to priority in *the* actual *tracing and mapping of the surfaces* of the principal part of *the British series of Strata*, and those scarcely less urgent calls, to support the cause of practical *English*

lish Geology, against the theoretical pretensions of an *Anglo-German Geognosy*, which has too high and proudly raised its intolerant head amongst us.

From the year 1791, or earlier, Mr. Smith possessed very superior opportunities for, and began assiduously to apply himself to the *practical* comparison of, the *underground measures* or Strata of extensive and deep Collieries, with *the surfaces* or bassets of these same Strata; and he succeeded, in tracing and mapping these surfaces and those of other strata lieing below and above them in the series, to a considerable extent, in connection, for considerable distances *around Bath and Bristol;* and in 1794, having had the opportunity of travelling with a Committee of the Somersetshire *Coal Canal Company*, through all the principal Coal districts of England, almost to the borders of Scotland, his discovery was completed, as to *the same order of superposition, the same general features, and the same organic remains, accompanying each stratum,* with which he had made himself acquainted near Bath, *through the whole of their definable course across the Island.*

Immediately from this time Mr. S. resolved on the design, of extending his Map around Bath and Bristol, *to the whole of England and Wales,* and as much of Scotland as he might be able; to which *Map*, during its progress, as well as to his growing *Collection of marked Specimens of each stratum and its imbedded extraneous Fossils*, his friends had the most free access, and whereby several scores of Persons in the West of England, and other parts, had every facility given them, of becoming thoroughly acquainted with the subject, before I had the good fortune to hear of Mr. S. or his pursuits, in the year 1800; and yet it has since appeared, that few if any one, of my present scientific acquaintances, had heard of Mr. Smith, before myself. Such is the difficulty opposed to *merit alone*, bringing a man into notice.

Mr. Smith's liberality in communicating his discoveries even went so far, as to draw up, at a Friend's house, in the year 1799, a List of all the principal Strata of England*, with the names he had adopted for each, in their ascertained order, with the most characteristic Reliquia of each, their several effects in producing springs of water, and a pretty copious list of the quarries and places, best adapted for examining each of these strata: of this List he gave some copies at the time to his Friends present, and they from time to time multiplied and distributed

* Last Sunday Evening (April 30) Mr. S. exhibited at Sir Joseph Banks's *Converzatione*, an exact copy of this his original List of Strata, and intends to publish the same.

them,

them, so that before the commencement of the present century, many copies of a list got into *private* circulation, which explained the chief results of Mr. S's arduous labours.

Soon after this period, Mr. S. printed and widely circulated a Prospectus, which further and pretty fully explained his discoveries and results, and the modes of their beneficial application to the concerns and interests of the Land and Mineral owners: but these appeals to the Public, failing to produce an adequate list of Subscribers to Mr. S's proposed Map and Work, and having *received no Money on account thereof*, but *from two Individuals*, he found himself compelled to lay the same aside, and apply very assiduously to his Engineery and other Business, for retrieving his pecuniary affairs, from the embarrassments that a too ardent zeal in the prosecution of this great and truly *national undertaking*, had brought on him!

In this dilemma of my Friend, and instructor in his new art of *Mineral Surveying*, and after I had fully ascertained the originality, as well as verified the truth and importance, of his discoveries relating to the mineral structure of Britain, of the south-east and east of England in particular, I saw with the utmost regret, persons starting up on every side, who had evidently either directly, or more indirectly, although not less certainly, *acquainted themselves with Mr. Smith's ideas and leading facts*, who were preparing to profit by them, and who yet appeared little disposed to acknowledge their obligations.

These circumstances, occasioned me almost incessantly to urge Mr. S. as others did whose advice ought to have had more weight, to bring out a publication on the subject, however short and in outline: but failing in accomplishing this, I determined to let no opportunity slip, of bringing Mr. S's discoveries and new professional practice sufficiently before the Public, *to secure him the credit* of them at least; and I began, in 1805, by showing at some length, their application to the important concerns of the *Civil Engineer*, in the article *Canal*, which I undertook to prepare for Dr. Rees's Cyclopædia*, and followed the same up,

* By a sort of fatality which attended my connection with this Work, it happened, that the Editor rejected nearly all this part of the manuscript, in his hasty and indiscreet mode of shortening my communication: I continued, however, to embrace the numerous opportunities that offered as a contributor to that work, for introducing the mention of Mr. Smith and his discoveries, and of making reference to future articles on other points, from the article *Clay Strata*, to that on *Joints* or fissures in the strata, inclusive: in the course of which communications, I prepared for the article *Geology*, a brief summary view of the theoretical results that appeared to me deducible, from *Mr. Smith's* discoveries and *my own* verifications thereof,

up, in occasional communications* to your and other periodical Publications, until 1811; when Mr. Smith, having yet published nothing on the subject, seemed in danger of lessening, if not perhaps being judged to have forfeited, his well merited claims, I gave such an abstract of the history of his proceedings and his results, in my Derbyshire Report, as seemed to me best calcu-

of, and I received some time after the delivery of this MS., assurances, that the same should appear, along with the accounts of the principal systems of Geology and Theories of the Earth, that had been previously published:—what, however, was my surprise and that of many others, on the appearance in 1810 of this article *Geology*, promised to be a very comprehensive and full one, to find a mere statement of the *Wernerian Theory*, with suitable flattery to its author; and consistently enough, this was prefaced, by such remarks as the following; viz.—This interesting *part of mineralogy* (the new Geognosy) principally owes the distinguished rank it now holds among *the Sciences*, to the celebrated Professor of Freyberg, who has separated *Geognosy* from *Geology*, "considering the latter as a merely speculative branch of knowledge, and as having nearly the same relation to the former, *which astrology has to astronomy!*"—The framers of most of these tissues of extravagant notions, known by the appellations of *theories of the earth*, have been satisfied with a very moderate share of materials for their structures—nothing is better calculated to flatter self-love than to be mentioned as the creator of a theory of the Earth—in short—"who would be desirous to waste his time in refuting, or even remembering *all* (*or any of*) *the theories of the Earth* now extant?"—(and doubtless the liberal *foreigner* in *British* pay, who wrote this, meant to insinuate)—or what Geognost would wish, to continue to be told, of anything that Englishmen have, or can do in Geology?—And accordingly it has occurred, throughout the articles since furnished, from this anti-*British* source, that Mr. Smith or any of his coadjutors or their discoveries, are not mentioned, or have scarcely any of the rapidly accumulating facts of the British stratification been brought forward, in this extensive English Dictionary, even where they had been promised *and referred to*, in some instances; but the Dogmas of the idol of Geognosts, Werner! and the observations and opinions of his followers abroad, with those of a few of the lately initiated amongst ourselves, have been held to be all-sufficient, for the information of English Readers!!

It gives me pleasure however to add, that the serious evil above complained of, seems at length to have worked its own cure, since on reading part of the particle *Rock*, in the last published leaf of this Work, the same plainly appears to be *from another pen*;—begins by restoring the Geological meaning of one of the most common and useful *English terms*, which inconsiderate Anglo-German Geognosts had endeavoured to alter, so essentially, as to make *rock* mean, very commonly, *a soft and earthy stratum*, like clay, marl, sand, and the like!—the infallibility of *the Geognosy*, as detailed in the article *Geology*, is plainly called in question;—and the writer, liberally assigns to Mr. S. the discovery, to which your pages, Mr. Editor, have so often stated him to be well entitled, with regard to the distribution of fossil shells and other organic remains in the Strata, and their uses in identifying the same, &c.

* The earliest and most explicit of these communications, was on the practicability of the proposed Archway under the Thames at Rotherhithe in 1806, in your xxvth volume, p. 44.

lated,

lated, *to put the Public usefully in possession of them*, and to secure the future credit of them for my valued, but rather untractable friend.

After some time, the Rev. Joseph Townsend published a 4to work, which, although under the very ill-chosen title of "The Character of Moses," &c. is almost entirely occupied with *local details*, respecting *the stratification of the West of England*, and therein he very handsomely, in the preface and other parts, acknowledges the having first *learnt to trace the Strata, and very many of his particulars concerning them, from Mr. Smith*, in the year 1801, and subsequently.

On the proposal for founding the *Geological Society* of London, in 1808, Mr. Smith and myself were often complimented, in the Letters and remarks of our Friends, on the great *éclat* which this new Institution would give *to Mineral Surveying*, as a chief basis on which British Geology should be built; our replies at the time were, that this result would altogether *depend, on the Persons* to whom *the management of the Society's concerns* might fall, and be retained; and the result has unfortunately corresponded,—instead of the least patronage or countenance being given to Mr. Smith, every means, direct and indirect, were soon resorted to, by a leading Individual therein, in particular, *to obtain his materials and delineate them on a new Map*, pretended, at first, to be for the private use of the Society; but after it had twice or thrice been copied, to correct its first egregious errors, as new materials were quickly collected, with inconceivably less pains or cost than Mr. Smith's materials were originally obtained, and I was repeatedly applied to for contributions to this new Map, I began to suspect, that all was not right, and determined on putting the question plainly, whether the design was not really entertained, *of publishing this rival Map?*, and this not being longer denied,—then, whether it was intended in such publication, to make *the acknowledgements* so justly due to Mr. Smith, for his long priority in the research, and his materials, obtained as above mentioned?, when I was unblushingly told, that theirs being a Map begun and altogether *made on Wernerian principles!*, no such acknowledgements as I asked, would be made!!

Much as I may have heard from various quarters, since Mr. John Cary seriously undertook *the publication*, of Mr. Smith's Map, (and became a useful sort of security to the Public, that he should not very long delay completing it,) on the progress then actually making in Engraving this pretended Geognostic Map (concerning which some curious history will remain yet to be disclosed) first in London and then in Paris, I had so much confidence

fidence that the fear of the exposure I could make, and the consequent shame and disgrace that must attach to the actors herein, would restrain them, that I had determined to delay submitting the present statements to the public, and should have continued to do so, but for a publication that has lately issued from the University press at Oxford, on Geology, dedicated to the worthy Geognost, to whom I have been principally alluding above; and wherein the author Dr. Kidd*, has at pages 21 to 27, given a brief account of *the strata of England*, essentially the same as Mr. Smith has been in the habit of explaining and showing them for twenty years past, and as I published them four years ago in the Derby Report, yet not a mention or hint of Mr. Smith or his labours, escapes this *impartial* Doctor!, (although he makes many references to the Derby Report), but he on the contrary, concludes his account thus; viz. "In the foregoing account of the strata of the SE part of this Island, I have been purposely very brief: not only because they have *already been described by other authors:* but because I have reason to believe, that a still *more detailed and accurate account of them is likely to be given* by a Gentleman (to whom he dedicates), who, from his extensive travels both *on the Continent* and in this Island, is *most competent to the task*, and to whose *private*† but persevering exertions, Geology has long been deeply indebted."

Now is it not surprising, that Dr. K., who makes *express references*, by volumes and pages, to 44 different works, for his Geological materials‡ (and refers to special few *of his own materials*)

* Who has himself, and some, if not all, of his coadjutors noticed by him, p. vii. examined Mr. Smith's Maps and Collection, long ago

† The fact here alluded to, of the Geognost here so extolled, having yet *published nothing* on Geological subjects, as far as I know, is worthy of observation by the impartial Reader, as well as the prevailing practice, of ascribing all Geognostic merit to M. Werner, who has alike *withheld his speculations from the Public*, on most of the theoretic points. that are known *to be his*, by the English Public, *only through the Writings of Mr. Jameson*, and others of his "admiring pupils:" and yet, the plea is almost daily brought forwards, as Mr. Smith's friends in general must have heard, by these Anglo-Wernerians, that it is perfectly fair, to anyhow obtain, *and to publish*, all that Mr. Smith has so long ago done, without giving him any credit for the same, "*because he has not published them*," say they!!: and I may add for these worthy Gents, that all which Mr. S. has done *in teaching persons*, and they *in publishing abstracts of his facts and discoveries*, should go for nothing in *his* favour, but the knowledge of these publications may, and ought as much as possible, *to be withheld* from the Public!!

‡ Dr. Kidd, presuming to sit in academic Judgement, as it were, on the pretension of *all the previous Writers* on Geological subjects, as to their *not having brought forward any perfect Evidence* (see his Title) *in support of a theory of the Earth*, has very unjustly overlooked and made no mention

terials) should have suppressed the mention, of all of those "other authors" (Query, Farey, Parkinson*, Sowerby, Townsend, Bakewell †, &c.?) who have briefly described the strata of England, if he meant to allude to *the publishers of Mr. Smith's results;* but that neither Mr. Smith or any of those who have had the liberality, publicly to bring forward or admit his claims on this head, were intended in all the paragraphs here quoted from Dr. K. will be apparent, from the well known fact of Mr. Smith *never having quitted this kingdom;* and also from the two very next paragraphs in Dr. K's Book, which, says he, contain "the earliest and *most interesting hints I have met with respecting a regularity in the succession of the strata of the Earth:*"—the first of these early and notable instances, is, a mere *suggestion,* on an unfounded opinion by Dr. Lister, in 1684, *for others to set about making* a coloured Map of the "upper *soils*" (whether *alluvial* or otherwise) of England, but which *no one attempted to carry into effect,* until the County Reports to the Board of Agriculture were undertaken, long since Mr. Smith's Map of the *Strata* (lieing *beneath* these superficial and alluvial *Soils*) was begun and well nigh finished.

The second of these interesting cases, quoted by Dr. K. is, that of Mr. *B. Holloway* (not *Mitchell* ‡, as the very reprehensible laxity of the Doctor's *quotations,* had said) who in 1722 mentioned, in a Letter to Dr. Woodward, that the Fuller's-Earth *Sand ridge* of *Woburn,* ranged through Shotover near Oxford, and *Newmarket-heath* near Cambridge, and accompanied "every-

tion of the recent work that I have quoted in p 337, by the Rev. Joseph Townsend, the respectable author of Travels in Spain: and for which no sufficient reason appears to me in the reading of Dr. K's Book, but the improper desire, more effectually to conceal Mr. Smith's claims, and *appropriate his discoveries to his Geognostic Friend!*

* Mr. James Parkinson's "Organic Remains," in 3 volumes 4to, containing towards its conclusion, a very explicit notice of Mr. Smith's discoveries and of his chief results; and Mr. James Sowerby's "Mineral Conchology," containing a great many local facts respecting the Strata of England and their organic contents, and in which due justice has been done to Mr. Smith, are alike passed over without mention, by the *impartial* Dr. K.!

† Mr. Bakewell's Introduction to Geology, 1st Edit. as being the last express work on the subject in this Country, is very reprehensibly overlooked by Dr. K; but this could not have been, on account of his declared partiality to Mr. Smith, as the Readers of your xliid and xliiid volumes must be sufficiently aware.—I would add, that the forthcoming 2nd Edition of Mr. B's work will, it is said (by himself), endeavour to make amends to Mr. Smith, for the neglect shown to him in the first: and perhaps certain Geognosts may have been much earlier apprized of this circumstance, than myself?.

‡ See the Phil. Mag. vol. xxxvi. p. 102, vol. xxxvii. p. 175 Note, and vol. xxxix. p. 91 Note.

 where

where at about the distance of eight or ten miles," the *Chalk ridge* of the Chiltern and Gog-magog Hills, near to the Cities above named.

Now it is observable, that neither Mr. Holloway or Dr. Woodward *then*, *or Dr. Kidd now* (at the distance of more than ninety years) have seemed to be at all aware, that *the Black-heath Sand* must be found on *Newmarket-heath*, which is *above* the Chalk (if there be *any regular stratum* of sand there?) whereas *the Woburn Sand*, is far *below* the Chalk (see Derby Report i. 111 and 112), and consequently, the two Ridges so described, if they existed, must cross one another like a × (the algebraic sign) instead of being ||, as was pretended.—Which, I would ask, is most to be deplored, *the ignorance still prevailing* in the chief *Seats of Learning* among us, as to *the most obvious Geological facts around them?*, or *the pitiful design* manifested in Dr. K's Book, in the revival of these excusable mistakes of former great Men, of depriving a deserving, although a non-academical Individual, of the merited rewards of his labours?. I have several other appeals to make, against Dr. K's modes of selecting evidences, as well as against his judicial sentences on different Geological points, but must reserve them for a season of more leisure, and hasten now to remark on Mr. Gilby's very extraordinary Letter in your last.

Professor Jameson, in issuing to the world, *his* first version of *The Geognosy*, of Werner, declared himself unable to determine the "formation" to which the Gypsum of Derbyshire (with its imbedding Red Marl) should be referred, because forsooth, "no *well educated* Geognost has ever communicated any observations regarding it;" and if we may credit Mr. G. H. Gilby in your 301st page, this great Geognost, remained in his perplexing dilemma, as to "the real position of the red ground," or Red Marl, until he fortunately read Mr. G's paper! in your xlivth volume, page 241.—That after so many persons had laboured at investigating and describing the Gypseous or Red ground of England, so many hundreds of pages of Wernerian Memoirs, Geognostic Transactions, Geognostic Annals, &c. had been published, he, Mr. G. should hear himself *publicly announced*, as the *first*, the *well-educated*, and the *happy* Geognost, who had at last succeeded in solving this very knotty problem! (and in *your* work too, Mr. Editor!) might well prove "peculiarly gratifying" to him, and may easily be supposed, to have given almost instant origin to the letter of severe rebuke to me, for having dared, in your previous number, page 167, 169, &c. to speak of the "position of the Red ground, in regard to the Coal and its accompanying strata," without expressly quoting *as my authority*, his said paper, *that had been so highly honoured!*

But

But to be serious, and to show that no wrong has been done by me to Mr. G., but the contrary, I beg to point out to him and your Readers, that so far from *he* having, "full half a year" *previous to me*, pointed out the unconformableness of the Red Marl, the reference that was so distinctly before him in p. 167 (viz. to p. 330 of your xliiid volume) will show, that *five months before any communication from him appeared*, I had not only pointed out, a new and perfect case of this unconformableness (unknown to him) at Bedworth in Warwickshire, but had distinctly and correctly referred to the Papers of Mr. John Strachey, published in 1719 and 1725, which described *his* (Mr. G's) whole case of the same nature in the vicinity of Bath and Bristol!; and also, that *I had* expressly *invited your Correspondents*, to make and communicate observations on this point. I might therefore, with far more justice than Mr. G., complain of his want of candour, in silently passing by my previous notice and invitation.

I have, however, higher charges to prefer against Mr. G. for endeavouring, by the authority of Mr. Jameson's name (I hope without his sanction?) to "manœuvre for himself a gloria," and "plume himself" with merit, that so very justly *belongs to Mr. Strachey*, long since dead, whose *accounts and two sections*, most distinctly mention and *show the overlieing and unconformable position of the Red Marle*, &c. on the inclined Coal-measures of that district:—and that Mr. G. made *this arrogant claim*, not entirely without a knowledge of what Mr. Strachey had done, must I think be inferred, from page 245 of his first paper, wherein he mentions the description of Stowey and Farrington Collieries (by Mr. Strachey) in vol. xxx. of Phil. Trans. for 1719, but ascribes the same to "a *Mr. Williams*," contrary to all the facts contained in the volume quoted!!.

In like manner, I cannot altogether acquit Mr. G. of want of candour at least, if not of injustice, towards my friend Mr. Smith, whose prior labours in the same field he could not be in the least ignorant of, from Mr. Townsend's work, which he so often refers to in his first paper; and if Mr. G. really went over the ground which he describes, it is next to impossible, I think, but that numbers of persons there, must have told him, that the same observations had been made, and a Map of the strata drawn, and which had been exhibited among them for many years by Mr. Smith; who did not secretly do this, in a corner, but made a point of attending the Bath Society's Meetings and other similar occasions of public resort, to freely exhibit and explain his Map and Sections of the district, now so long after claimed, *to be first truly described by Mr. G.!!*

Mr. Smith was from the first aware of, and always taught, the correctness of Mr. Strachey's representations (with which *he* first made me acquainted) *in the places described by that Gentleman*, but Mr. Smith knew a great deal more than was known to Mr. Strachey, viz. that the coal-seams which near their bassets were highly inclined to the Red Marl, which lay upon their edges, by degrees became more flat and parallel to the Marl, as Steam-engines enabled them to follow these seams into the deep, and the inference seemed a natural one, that still further eastward, they would be found *parallel to the Red Marl and Lias upon it*.

In addition to these new facts, adverse to the generalization of Mr. Strachey's local observations on unconformable Marl, &c. the *pudding-stones** which were generally mentioned, as contained in these overlieing strata, not as continuous and regular strata, but varying considerably in thickness and being often wanting altogether, appeared to me *so very like to alluvial matters*, that, never having had the opportunity of minutely examining any place in the district myself, *I always doubted*, and so have uniformly expressed myself, that Mr. Strachey's really did form a case of *unconformable* regular or *undisturbed strata* †, until the same was *rendered more probable*, by an able pupil of Mr. Smith's, Mr. Bevan, having brought to light a case (see vol. xliii. p. 330) admitting of no doubt, as I happen sufficiently to know, from having myself been upon and examined the surface with him, several years before, when on Canal business near Bedworth, but at which time, the slight inquiries we made, as to the coal strata and works below, did not bring out *the least hint of any unconformableness*, or other deviation there from what is common in Coal Pits, everywhere!—in p. 174, I have alluded to similar, although apparently accidental difficulties, in gaining correct, or rather, perhaps, sufficiently full information,

* Some of these are by Mr. Gilby called "limestone breccia," and seem to me now referable, *to the unconformable conglomerate Yellow Limestone*, which I have since mentioned at Alberbury, Barnby, &c. page 168 of your March number: and had I when writing that page, happened to have turned to and reconsidered Mr. Gilby's description of these Limestone beds, in p. 246, I certainly should have referred thereto.

† These difficulties may very well be supposed to have prevented *Mr. Townsend* from so decidedly mentioning *the unconformableness* of the Red Marl, *as a general and unvarying fact*, as Mr. G. apparently would have had him done, and has himself done, in contradiction to the ascertained facts above mentioned, and in defiance of the reasoning which I have adduced, in the middle of page 170, to show that *the unconformableness* of the Marl or any other stratum, *can only show itself* in the very few spots, comparatively, *where edges of strata are covered by it, and not where the overlieing stratum covers the plane of a previously deposited stratum*, (see Note † on page 170).

respecting

respecting Garforth, Kippax, and Glass-Houghton Collieries; all which I think might have restrained the sarcastic jeers of the *well educated* Mr. G. at *practical* Men, remaining long in the dark, as to facts, that seem very plain, after being fully known.

It may be proper to add in this place, that not only the majority, but nearly all of the local and operative Colliers in England (and too many of their superiors) except some near Bath and Bristol and near Bedworth, still doubt, and even deny the overlieing of the Red Marl or its Red Rock on Coal-measures, either conformably or otherwise, and that they generally assert, either, as I have mentioned of the Ashby-de-la-Zouch Coal-field, Derby Rep. i. 174*, that a *fault* is always met with, before reaching the edge of the Red Marl or Red Rock; or, that the seam they have been working, rises suddenly up and bassets or "is thrown out," before reaching such edge, or more commonly, they say, that it is "cut off," without being able to explain how; and they mostly aver, that in no case do the Coals pass under the Red Marl or Red Rock! These representations seem to have been often made to Mr. Bakewell, and have been and will, for a time at least, continue to be so repeated, to every inquirer. In Durham, it should seem, that a similar notion yet prevails, and was lately mentioned to Dr. Thomson and Mr. Bakewell (see p. 177 of this volume), that the Coals never pass under the yellow Limestone!

In saying thus much, I hope no one will consider me as casting reflections, as to the veracity or the designs of this body of highly useful and respectable Men, far from it; I have often had the satisfaction of saying, that on *matters of fact*, I rarely if

* Since my Report was printed, and since I have had any opportunity of writing on the subject of this Coal-field, I have had numerous and excellent opportunities of observing and of studying the effects of Ridges and Troughs, Bumps and Hollows and other *original inequalities in the planes and thicknesses of strata*, through which *Faults have subsequently broken*; and am now well convinced, that practical Colliers are very often if not mostly wrong, when they refer the locally *rapid dips* they meet with, *to* or *from* a fault, to *a tilt* of one or both of the piles of strata contiguous to such fault, and consider such tilts *to be occasioned by the Fault*; and that being formerly misinformed on these points, I have involved my generalization or *theory of Faults*, in its chief difficulty, that of the very *wedge-like form* of the Fault-stuff (Derby Rep. i. p. 121): from which I could now clear it, and render it applicable, perhaps to all the cases that really occur in the Collier's practice, if I had sufficient leisure or inducement to set about such a work:—until then, I must continue to describe and repeat each case, where necessary, in my private or professional Reports on Collieries, instead of being able to shorten them, by reference to a published work, describing each case.

 ever

ever find myself at issue with the *practical* Colliers, Miners, &c. but on matters of *inference*, or involving their belief, of things *not actually seen by themselves*, I almost daily, when on my Mineral Surveys, find myself point blank at issue with them, and so have been obliged to continue, in numerous instances;—I am, I hope, alike incapable of being influenced by *numbers*, to adopt or reject any position or deduction to be made from the study of Geological phænomena, as I am of yielding in any such cases to *authority*, however academical or imposing its aspect may be; nor will I stand quietly by and see, a most deserving Individual and Friend, deprived of the just reputation due to his labours and discoveries, or neglect the attempt, at contributing towards his more solid reward. And I am,

Sir,

Your obedient servant,

12, Upper Crown Street, Westminster, May 3, 1815. JOHN FAREY, Sen.

LXII. *Letter from M.* AMPERE *to Count* BERTHOLLET, *on the Determination of the Proportions in which Bodies are combined, according to the respective Number and Arrangement of the Molecules of which their integrant Particles are composed.*

[Concluded from p. 193.]

WE may also deduce from this manner of conceiving the composition of bodies, the relations of the quantities of acid, basis, and even of water of crystallization, which ought to be found in the acid salts, the neutral, or those that are hypersaturated with one and the same species, according to the representative forms of the particles of the acid and the base. It is thus, for instance, that we find, according to that of the particles of the sulphuric acid, that most of the supersaturated sulphates ought, conformably to experience, to contain three times more bases than the neutral sulphates, and that the quantity of sulphuric acid is double in the acid sulphates to what it is in the neutral sulphate; whereas the sulphurous acid may, according to the representative form of its particles, make with ammonia an acid salt, into which it enters in greater quantity than into the neutral sulphite, in the ratio of three to two only. Such, in short, is the acid sulphite which we obtain by distilling the neutral sulphate of ammonia.

I shall not enter here into the details contained in my full memoir on the different combinations of ammoniacal gas with the other acid gases: the accordance of the results to which we are

are led, with those of the experiment, seems to me one of the most remarkable proofs of the theory therein detailed: but in order to give an example of the manner in which we may draw from this theory the determination of the quantity of water which is combined with bodies, either in the state of crystallization or even after they have undergone the action of a strong heat, I shall cite the determination according to the representative form of the particles of potash from the quantity of water which is united with it in these two states. After having established, setting out from the phænomena which potassium exhibits when we place it in contact with water and ammoniacal gas, that the particles of potash have as their representative form an octohedron composed of two molecules of oxygen and four of metal, I find that, in the crystallized hydrate, the quantity of the oxygen of the water ought to be double that which is united to the potassium; but after the hydrate has been fused, those two quantities of oxygen ought to be as 4 : 3, because a particle of hydrate in this state has for its representative form a heptaoctohedron formed by the meeting of a trioctohedron composed of three octohedral particles of potash and of a tetraoctohedron of four octohedral particles of water. Now, according to the composition of potash as determined by Messrs. Thenard and Gay Lussac, 100 parts of potassium unite with 19·945 parts of oxygen to make 119·945 of potash. It follows from what I have said, therefore, that this quantity of potash ought to retain, at every temperature, a quantity of water in which there is 26·593 of oxygen, and which consequently weighs 30·139; that is to say, nearly $\frac{1}{400}$th of the weight of the potash, as has been found by the most accurate analyses.

The combinations of oxygen, hydrogen, and chlore, either with themselves or with other bodies, have been successively the subjects of researches analogous to those just mentioned. As it is impossible to indicate all the results here, I shall confine myself to those of the combinations in which all the elements may be obtained in the state of gas, and in which the numbers of the molecules of each of their elements are consequently given immediately.

We have already ascertained the representative forms of the particles of two combinations of azote and of oxygen, the oxide of azote and the nitrous gas; that of the nitrous acid ought to be determined according to the ratio of the volumes of nitrous gas and oxygen of which it is composed. Experiments have been made upon this subject, but their results are at variance. According to the analyses of Sir Humphry Davy, this acid is composed of two volumes of nitrous gas, and of a volume of oxygen: each of its particles will then contain two molecules

of

of oxygen more than the particles of nitrous gas, and will consequently have, as the representative form, an octohedron composed of two molecules of azote and four of oxygen; but then, as in all the other combinations where the volume of one of the components is double that of the other, the volume of nitrous gas will not change by the addition of oxygen: the greatest condensation which takes place appears to me to be ascribable to this, namely, that in proportion as these octohedrons are formed, they are combined in hexa-decahedrons, with tetrahedrons of nitrous gas. As two molecules of oxygen are then sufficient for the formation of one of these hexa-decahedrons into which two entire particles of nitrous gas enter, the volume of oxygen is only the fourth part of that of the nitrous gas, and the volumes of azote and oxygen are, in the nitrous acid, as 4:6. These results agree with the experiments of M. Berzelius. On this hypothesis, the condensation ought to be 3-fifths of the total volume; but it will not take place completely except when, the oxygen being introduced by small portions into the nitrous gas, the octohedrons just mentioned, in proportion as they shall be formed, will meet an excess of tetrahedron of nitrous gas with which they may be combined. If we introduced, on the contrary, the nitrous gas into oxygen, a part of these octohedrons might remain isolated, and there might result combinations and condensations in variable proportions.

It follows from the composition of the nitric acid, as determined by Sir H. Davy, and which is confirmed by the decomposition of the nitrate of ammonia, that a particle of this acid, if we can obtain it without water, will be composed of one particle of azote and of two particles and a half of oxygen. It will then contain four molecules of azote and ten of oxygen; and we may conceive it as formed by the meeting of two tetrahedrons of nitrous gas joined to an octohedron of six molecules of oxygen *, and forming with it a dodecahedron. But in the combination which this acid always forms with water, we must suppose that the octohedron of oxygen and two octohedrons of water form a trioctohedron which is united in a trapezoidal form with the two tetrahedrons of nitrous gas: we may hence conclude what is the quantity of water in the most highly concentrated nitric acid, and we find by calculation that it is nearly what Dr. Wollaston has determined by his experiments.

In the nitrate of ammonia, a particle of dry nitric acid is united to two particles of ammoniacal gas; so that one particle

* We may also suppose that, in the formation of the nitric acid, the hexa-decahedron of nitrous acid is joined to a tetrahedron of oxygen, which always forms a combination of an octohedron with two tetrahedrons, and changes nothing in the following explanations.

of

of salt is formed by the junction of one octohedron of oxygen, two tetrahedrons of nitrous gas, and four tetrahedrons similar to those which enter, to the number of two, into each particle of ammoniacal gas: the representative form of this particle is therefore a pyramidated hexa-tetrahedron, containing ten molecules of oxygen, eight of azote, and twelve of hydrogen. When we decompose the salt by heat, the eight molecules of azote form two particles of oxide of azote with four molecules of oxygen, and the twelve molecules of hydrogen form three particles of water with the six other molecules of oxygen.

When the salt contains besides water of crystallization, we ought to obtain more than three particles of water; but in all cases, we can only extract from its decomposition water and oxide of azôte, as we find by experience.

If the quantity of water of crystallization was equal in the salt to that which is contained in the most highly concentrated nitric acid, it would be necessary to join to the octohedron and to the six tetrahedrons of which one of the particles is composed, two other octohedrons of water; which will give for the representative form of the crystallized nitrate of ammonia, an octocentahedron formed by the meeting of six tetrahedrons and one trioctohedron. Chlore is combined with hydrogen in equal volume, and the muriatic acid gas which results occupies a volume equal to the sum of the volumes of those two component parts. We might account for this mode of combination, by supposing that the representative forms of the particles of chlore are isolated tetrahedrons like those of oxygen, azote, and hydrogen; that of the particles of the muriatic acid will then be a tetrahedron: but we may also explain it by considering each particle of chlore as formed by the meeting of two tetrahedrons in a parallelopipedon, and as consequently containing eight molecules. This last hypothesis is the only one which can agree with the proportions of the other combinations of chlore, the phænomena which they exhibit, and the properties which characterize them.

By admitting it, we find that each particle of muriatic acid, containing the half of a particle of hydrogen and the half of a particle of chlore, has for its representative form an octohedron composed of two molecules of hydrogen and four molecules of chlore. When the muriatic gas is combined with the ammoniacal gas, each of its octohedrons is combined with a cubic particle of this gas: hence it follows, that it ought to absorb of it a volume equal to its own, as experience shows, and that the particles of the salt thus formed ought to have as their representative form a rhomboidal dodecahedron: this form is, in fact, one of those which belong to the system of crystallization of

of sal ammoniac, and all the others might consequently be referred to it by different decrements. The acid gases, the particles of which have a cube for their representative form, tend, on the contrary, to combine with the ammoniacal gas in such a way that the volume of one of the gases is double that of the other, because the most simple polyhedron which we can form with cubes is the hexa-tetrahedron which contains three of them.

The composition of the gas formed by the meeting of oxygen and chlore, which Sir Humphry Davy has discovered and called euchlorine, is one of the most remarkable, from the proportions in volumes of its two component parts. According to his analysis, five volumes of the gas which he submitted to the experiment, gave, on being decomposed by heat, two volumes of oxygen and four of chlore. These relations seem contrary to all analogy, and they seem to be inadequate to the explanation of the composition of the particles of euchlorine, without admitting that the gas analysed by this celebrated chemist was mixed with a little chlore; a supposition which naturally occurs when we recollect that the process by which this gas was obtained gave a mixture of euchlorine and chlore, from which this last gas was separated by shaking it over mercury; a process which probably did not take up all the chlore, and which, besides, did not leave any method of ascertaining, even if successful, that the residue of this operation was pure euchlorine.

I think therefore that we must account for this analysis, by supposing that the gas employed contained one-fifth of chlore; and that, of the five volumes submitted to the experiment, there were four only of a gas really composed of oxygen and chlore. By supposing that the representative form of its particles is a cube composed of two molecules of oxygen and five of chlore, we find that four particles of this gas ought to contain eight molecules, *i. e.* two particles of oxygen and twenty-four molecules, *i. e.* three particles of chlore: so that the decomposition of four volumes of pure euchlorine would produce, upon this hypothesis, two volumes of oxygen and three volumes of chlore. These three volumes of chlore united to a volume of the same gas, which formed by its mixture with the four volumes of euchlorine the five volumes which were operated upon, ought to have given in the residue the four volumes of chlore found by Sir Humphry Davy.

The relation of three volumes of chlore and two volumes of oxygen in the euchlorine, seems at first to present no analogy with the relations which we observe in the combinations of the other gases; but this anomaly is only apparent, and merely arises from the tetrahedrons of the chlore, instead of being separated like the tetrahedrons from the oxygen, the hydrogen, and

and the azote, remaining combined by pairs in each particle of chlore; so that a volume of this gas is equivalent to two volumes of another gas relative to combinations; and that if the tetrahedrons of the chlore shall all be separated from each other, we shall obtain, by the decomposition of the euchlorine, six volumes of chlore and two volumes of oxygen, precisely as we find in the residue from the decomposition of ammoniacal gas, the particles of which have the same representative form with that of the euchlorine, six volumes of oxygen and two of azote.

The results which I have just indicated form but a very small part of those which we may deduce from the consideration of the representative forms of the particles of bodies applied to the determination of the proportions of inorganic compounds. The chemistry of organized bodies also presents numerous application of this theory; but it is in this respect particularly that there are many analyses and calulations to make for completing it. I have nevertheless drawn several determinations relative to the composition of different circumstances drawn from the vegetable kingdom, which agree too strongly with the results of experience to leave any doubts as to the utility of which it may be in this part of chemistry.

LXIII. *Some Experiments and Observations on the Colours used in Painting by the Ancients.* By Sir HUMPHRY DAVY, *LL.D. F.R.S.**

I. *Introduction.*

THE importance the Greeks attached to pictures, the estimation in which their great painters were held, the high prices paid for their most celebrated productions, and the emulation existing between different states with regard to the possession of them, prove that painting was one of the arts most cultivated in ancient Greece: the mutilated remains of the Greek statues, notwithstanding the efforts of modern artists during three centuries of civilization, are still contemplated as the models of perfection in sculpture; and we have no reason for supposing an inferior degree of excellence in the sister art, amongst a people to whom genius and taste were a kind of birthright, and who possessed a perception, which seemed almost instinctive, of the dignified, the beautiful, and the sublime.

The works of the great masters of Greece are unfortunately entirely lost. They disappeared from their native country during the wars waged by the Romans with the successors of Alexander, and the later Greek republics; and were destroyed either

* From the Philosophical Transactions for 1815, part i.

either by accident, by time, or by barbarian conquerors at the period of the decline and fall of the Roman Empire.

The subjects of many of these pictures are described in ancient authors, and some idea of the manner and style of the Greek artists may be gained from the designs on the vases, improperly called Etruscan, which were executed by artists of Magna Græcia, and many of which are probably copies from celebrated works: and some faint notion of their execution and colouring may be gained from the paintings in fresco found at Rome, Herculaneum, and Pompeii.

These paintings, it is true, are not properly Greek; yet, whatever may be said of the early existence of painting in Italy as a native art, we are certain that, at the period when Rome was the metropolis of the world, the fine arts were cultivated in that city exclusively by Greek artists, or by artists of the Greek schools. By comparing the descriptions of Vitruvius* and Pliny with those of Theophrastus†, we learn that the same materials for colouring were employed at Rome and at Athens; and of thirty great painters that Pliny mentions whose works were known to the Romans, two only are expressly mentioned as born in Italy, and the rest were Greeks. Ornamental fresco painting was indeed generally exercised by inferior artists; and the designs on the walls of the houses of Herculaneum and Pompeii, towns of the third or fourth order, can hardly be supposed to offer fair specimens of excellence, even in this department of the art: but in Rome, in the time of her full glory, and in the ornaments of the imperial palace of the first Cæsars, all the resources of the distinguished artists of that age were probably employed. Pliny names Cornelius Pinus and Accius Priscus as the two artists of the greatest merit in his own time, and states that they painted the Temple of Honour and Virtue‡, "Imperatori Vespasiano Augusto restituenti;" and it is not improbable that these artists had a share in executing, or directing the execution of, the paintings and ornaments in the baths of Titus; and at this period the works of Zeuxis, Parrhasius, Timanthes, Apellés, and Protagoras were exhibited in Rome, and must have guided the taste of the artists. The decorations of the baths were intended to be seen by torch-light, and many of them at a considerable elevation, so that the colours were brilliant and the contrast strong; yet still these works are regarded by connoisseurs as performances of considerable excellence: the minor ornaments of them have led to the foundation of a style in painting which might with much more propriety be called Romanesque than Arabesque: and no greater eulogy can be bestowed upon them

* De Architectura, lib. vii. cap. 5. † De Lapidibus.

‡ Plin. Nat. Hist. lib. xxxv. cap. 37.

than

than the use to which they have been applied by the greatest painter of modern times, in his exquisite performances in the Vatican. In these and in other works of the same age, the effect of the ancient models is obvious; and the various copies and imitations that have been made of these remains of antiquity have transferred their spirit into modern art, and left little to be desired as to those results which the skill of the painter can command. There remains, however, another use to which they may be applied, that of making us acquainted with the *nature* and *chemical composition* of the colours used by the Greek and Roman artists. The works of Theophrastus, Dioscorides, Vitruvius, and Pliny, contain descriptions of the substances used by the ancients as pigments; but hitherto, I believe, no experimental attempt has been made to identify them, or to imitate such of them as are peculiar*. In the following pages I shall have the honour of offering to the Society an investigation of this subject. My experiments have been made upon colours found in the baths of Titus, and the ruins called the baths of Livia, and in the remains of other palaces and baths of ancient Rome, and in the ruins of Pompeii. By the kindness of my friend, the celebrated Canova, who is charged with the care of the works connected with ancient art in Rome, I have been enabled to select, with my own hands, specimens of the different pigments that have been found in vases discovered in the excavations lately made beneath the ruins of the palace of Titus, and to compare them with the colours fixed on the walls or detached in fragments of stucco: and Signor Nelli, the proprietor of the Nozze Aldobrandine, with great liberality permitted me to make such experiments upon the colours of this celebrated picture, as were necessary to determine their nature. When the preservation of a work of art was concerned, I made my researches upon mere atoms of the colour, taken from a place where the loss was imperceptible: and without having injured any of the precious remains of antiquity, I flatter myself, I shall be able to give some information not without interest to scientific men as well as to artists, and not wholly devoid of practical applications.

* In the 70th volume of the *Annales de Chimie*, page 22, M. Chaptal has published a paper on seven colours found in a colour-shop at Pompeii. Four of these he found to be natural colours, ochres, a specimen of Verona green, and one of pumice stone. Two of them were blues, which he considers as compounds of alumine and lime with oxide of copper, and the last a pale rose colour, which he regards as analogous to the lake formed by fixing the colouring matter of madder upon alumine. I shall again refer to the observations of M. Chaptal in the course of this paper. It will be found on perusal, that they do not supersede the inquiry mentioned in the text.

II. *Of*

II. *Of the Red Colours of the Ancients.*

Amongst the substances found in a large earthen vase filled with mixtures of different colours with clay and chalk, found about two years ago in a chamber at that time opened in the baths of Titus, are three different kinds of red; one bright and approaching to orange, another dull red, a third a purplish red*. On exposing the bright red to the flame of alcohol, it became darker red; and on increasing the heat by a blow-pipe, it fused into a mass having the appearance of litharge, and which was proved to be this substance by the action of sulphuric and muriatic acids. This colour is consequently minium, or the red oxide of lead.

On exposing the dull red to heat, it became black, but on cooling recovered its former tint. When heated in a glass tube it afforded no volatile matter condensible by cold but water. Acted on by muriatic acid, it rendered it yellow; and the acid, after being heated upon it, yielded an orange-coloured precipitate to ammonia. When fused with hydrate of potassa, the colour rendered it yellow; and the mixture acted on by nitric acid afforded silica and orange oxide of iron. It is evident from these results that the dull red colour is an iron ochre.

The purplish red submitted to experiments, exhibited similar phænomena, and proved to be an ochre of a different tint.

In examining the fresco paintings in the baths of Titus, I found that these colours had been all of them used, the ochres particularly, in the shades of the figures, and the minium in the ornaments on the borders.

I found another red on the walls, of a tint different from those in the vase and much brighter, and which had been employed in various apartments, and formed the basis of the colouring of the niche and other parts of the chamber in which the Laocoon is said to have been found. On scraping a little of this colour from the wall, and submitting it to chemical tests, it proved to be vermilion or cinnabar, and on heating it with iron filings, running quicksilver was procured from it.

I found the same colour on some fragments of ancient stucco in a vineyard near the pyramidal monument of Caius Cestius.

In the Nozze Aldobrandine, the reds are all ochres. I tried on these reds the action of acids, of alkalies, and of chlorine, but could discover no traces either of minium or vermilion in this picture.

Minium was known to the Greeks under the name of σανδαράχη †, and to the Romans under that of *cerussa usta*. It is said,

* Nearly of the same tint as prussiate of copper.

† Dioscorides, lib. v. 122.

by

by Pliny*, to have been discovered accidentally by means of a fire that took place at the Piræus at Athens. Some ceruse which had been exposed to this fire was found converted into minium, and the process was artificially imitated: and he states that it was first used as a pigment by Nicias†.

Several red earths used in painting are described by Theophrastus, Vitruvius‡, and Pliny. The Sinopian earth, the Armenian earth, and the African ochre, which had its red colour produced by calcination.

Cinnabar or vermilion was called by the Greeks κιννάβαρι §, and by the Romans *minium*. It is said by Theophrastus || to have been discovered by Callias, an Athenian, ninety years before Praxibulus, and in the 349th year of Rome, and was prepared by washing the ores of quicksilver. According to Pliny ¶, who quotes Verrius, it was a colour held in great esteem in Rome at the time of the Republic; on great festivals it was used for painting the face of Jupiter Capitolinus, and likewise for colouring the body of the victor in the triumphal processions, "sic Camillum triumphasse**." Pliny mentions that even in his time vermilion was always placed at triumphal feasts amongst the precious ointments; and that the first occupation of new censors of the Capitol was to fill the place of vermilion-painter to Jupiter.

Vermilion was always a very dear colour amongst the Romans; and we are informed by Pliny, that to prevent the price from being excessive it was fixed by the government. The circumstance of the chambers in the baths of Titus being covered with it, affords proof in favour of their being intended for imperial use; and we are expressly informed by the author I have just quoted, that the Laocoon, in his time, was in the palace of Titus††: and the taste of the ancients in selecting a colour to give full effect to their master-pieces of sculpture was similar to that of a late celebrated English connoisseur.

Pliny describes a second or inferior sort of vermilion formed by calcining stone found in veins of lead. It is evident that this substance was the same as our minium, and the Roman cerussa usta, and the stones alluded to by Pliny must have been carbonate of lead: and he states distinctly, that it is a substance which becomes red only when burnt.

* Lib. xxxv. cap. 20. † Id. ibid.
‡ De Architectura, lib. vii. cap. 7. § Dioscorides, lib. v. cap. 109.
|| De Lapid. cap. 104.
¶ Lib. xxxiii. cap. 36. Nunc inter pigmenta magnæ auctoritatis, et quondam apud Romanos non solum maximæ, sed etiam sacræ. ** Ibid.
†† Lib. xxxvi. cap. 4. Sicut in Laocoonte, qui est in Titi Imperatoris domo, opus omnibus et picturæ et statuariæ artis præponendum.

III. *Of the Yellows of the Ancients.*

A large earthen pot found in one of the chambers of the baths of Titus contains a quantity of a *yellow paint*, which, submitted to chemical examination, proved to be a mixture of yellow ochre with chalk or carbonate of lime.

This colour is used in considerable quantities in different parts of the baths; but principally in the least ornamented chambers, and in those which were probably intended for the use of the domestics. In the vase to which I alluded in the last section, I found three different yellows; two of them proved to be yellow ochres mixed with different quantities of chalk, and the third a yellow ochre mixed with red oxide of lead, or minium.

The ancients procured their yellow ochre * from different parts of the world; but the most esteemed, as we are informed by Pliny, was the Athenian ochre; and it is stated by Vitruvius, that in his time the mine which produced this substance was no longer worked.

The ancients had two other colours, which were orange or yellow; the *auripigmentum*, or *ἀρσενικον*, said to approach to gold in its colour, and which is described by Vitruvius † as found native in Pontus, and which is evidently sulphuret of arsenic; and a *pale* sandarach, said by Pliny to have been found in gold and silver mines, and which was imitated at Rome by a partial calcination of ceruse, and which must have been massicot, or the yellow oxide of lead mixed with minium. That there was a colour called by the Romans *sandarach*, different from pure minium, is evident from what Pliny says; namely, that the palest kind of orpiment resembles sandarach, and from the line of Nævius, one of the most ancient Latin poets, "Merula sandaracino ore:" so that this colour must have been a bright yellow similar to that of the beak of the blackbird ‡. Dioscorides describes the best *σανδαράχη* as approaching in colour to vermilion §, and the Greeks probably always applied this term to minium; but the Romans seem to have used it in a different sense; and some confusion was natural when different colours were prepared from the same substance by different degrees of calcination.

I have not detected the use of orpiment in any of the ancient fresco paintings; but a deep yellow approaching to orange, which covered a piece of stucco in the ruins near the monument of Caius Cestius, proved to be oxide of lead, and consisted of massicot mixed with minium. It is probable that the ancients used many colours from lead of different tints between the usta of

* ὤχρα, Theophrastus de Lapidibus. † Vitruvius, lib. vii.
‡ Histoire de la Peinture ancienne, page 199. § Lib. v. 122.

Pliny,

Pliny, which was our minium, and imperfectly decomposed ceruse, or pale massicot.

The yellows in the Aldobrandini picture are all ochres. I examined the colours in a very spirited picture, on the wall of one of the houses at Pompeii, of a lion and a man; they all proved to be red and yellow ochres.

IV. *Of the Blue Colours of the Ancients.*

Different shades of blue are used in the different apartments of the baths of Titus, and several very fine blues exist in the mixtures of colours to which I have referred in the last two sections.

These blues are pale or darker, according as they contain larger or smaller quantities of carbonate of lime; but when this carbonate of lime is dissolved by acids, they present the same body colour, a very fine blue powder similar to the best smalt or to ultramarine, rough to the touch, and which does not lose its colour by being heated to redness; but which becomes agglutinated and semifused at a white heat.

This blue I found was very little acted on by acids. Nitromuriatic acid by being long boiled upon it gained, however, a slight tint of yellow, and afforded proofs of the presence of oxide of copper.

A quantity of the colour was fused for half an hour with twice its weight of hydrate of potassa; the mass, which was blueish green, was treated by muriatic acid in the manner usually employed for the analysis of siliceous stones, when it afforded a quantity of silica equal to more than 3-5ths of its weight. The colouring matter readily dissolved in solution of ammonia, to which it gave a bright blue tint, and it proved to be oxide of copper. The residuum afforded a considerable quantity of alumine, and a small quantity of lime.

Amongst some rubbish that had been collected in one of the chambers of the baths of Titus, I found several large lumps of a deep blue frit, which when powdered and mixed with chalk produced colours exactly the same as those used in the baths, and which when submitted to chemical tests were found to be the same in composition.

The minute quantity of lime found in this substance was not sufficient to account for its fusibility: it was therefore reasonable to expect the presence of a fixed alkali in it; and on fusing some of it with three times its weight of boracic acid, and treating the mass with nitric acid and carbonate of ammonia, and afterwards distilling sulphuric acid from it, I procured from it sulphate of soda; which proves that it was a frit made by means of soda, and coloured with oxide of copper.

The undiluted colour in its form of frit is used for ornamenting

some

some of the mouldings detached from the ceilings of the chambers in the baths of Titus: and the walls of one chamber between the compartments of red marble, bear proofs of having been covered with this frit, and retain a considerable quantity of it.

There is every reason to believe that this is the colour described by Theophrastus as discovered by an Egyptian king *; and of which the manufactory is said to have been anciently established at Alexandria.

Vitruvius speaks of it, under the name of *cæruleum* †, as the colour used commonly in painting chambers, and states, that it was made in his time at Puzzuoli, where the method of fabricating it was brought from Egypt by Vestorius; he gives the method of preparing it by heating strongly together sand, flos nitri ‡, and filings of copper.

Pliny mentions other blues, which he calls species of sand (*arenæ*) from the mines of Egypt, Scythia, and Cyprus. These natural blues, there is reason to believe, were different preparations of lapis lazuli, and of the blue carbonates and arseniates of copper.

Both Pliny and Vitruvius speak of the Indian blue, which the first author states to be combustible, and which was evidently a species of idigo.

I have examined several blues in the fragments of fresco painting from the ruins near the monument of Caius Cestius. In a deep blue approaching in tint to indigo, I found a little carbonate of copper, but the basis of this colour was the frit before described.

The blues in the Nozze Aldobrandine, from their resisting the action of acids, and from the effects of fire, I am inclined to consider as composed of the Alexandrian or Puzzuoli blue.

In an excavation made at Pompeii, in May 1814, at which I was present, a small pot containing a pale blue colour was dug up, which the exalted personage, by whose command the excavation was made, was so good as to put into my hands. It proved to be a mixture of carbonate of lime with the Alexandrian frit §.

Vitruvius states, that the ancients had a mode of imitating the Indian blue or indigo, by mixing the powder of the glass called by the Greeks ὕαλος, with selinusian "creta" or annularian "creta," which was white clay or chalk mixed with stained glass; the same practice is likewise referred to by Pliny.

* De Lapidibus, sect. xcviii. † Lib. vii. cap. 11.

‡ This identifies the nitrum of the ancients with carbonate of soda.

§ This probably is the same colour as that examined by M. Chaptal. He did not search in it for alkali, or there is every reason to suppose he would have found soda.

There

There is much reason for supposing that this stained glass, or ὕαλος, was tinged with oxide of cobalt; and that these colours were similar to our smalt. I have not found any powdered colour of this kind in the baths of Titus, or in any other Roman ruins; but a blue glass tinged with cobalt is very common in those ruins, which when powdered forms a pale smalt.

I have examined many pastes and glasses that contain oxide of copper; they are all blueish green, green, or of an opaque watery blue. The transparent blue glass vessels which are found with the vases in the tombs in Magna Græcia are tinged with cobalt; and on analysing different ancient transparent blue glasses which Mr. Millingen was so good as to give me, I found cobalt in all of them*.

Theophrastus, in speaking of the manufacture of glass, states as a report that "χαλκὸς" was used to give it a fine colour, and it is extremely probable that the Greeks took cobalt for a species of χαλκὸς. I have examined some Egyptian pastes which are all tinged blue and green with copper; but though I have made experiments on nine different specimens of ancient Greek and Roman *transparent* blue glass, I have not found copper in any, but cobalt in all of them†.

V. *Of the ancient Greens.*

The ceiling of the chambers called the Baths of Livia is highly ornamented with gilding and paintings; the larger paintings have been removed, but the ground-work and the borders remain. A fragment detached from the borders, which appears of the same colour as the ground-work, was of a deep sea green. The colouring matter examined, proved to be soluble in acids with effervescence; and when precipitated from acids, it redissolved in solution of ammonia, giving it the bright blue tint produced by oxide of copper. There are several different shades of green employed in the baths of Titus, and on the fragments found near the monument of Caius Cestius: in the vase of mixed colours I found three different varieties; one, which approached to olive, was the common green earth of Verona; another, which

* The mere fusion of these glasses with alkali and digestion of the product with muriatic acid was sufficient to produce a sympathetic ink from them; even the silica separated by the acid gained a faint blue green tint by heat, and the solution in muriatic acid became permanently green by the action of sulphuric acid, a phenomenon Dr. Marcet has observed as belonging to the muriate of cobalt.

† A gentleman at Milan informed me last summer, that he had found oxide of cobalt in the blue glass found in the ruins of Hadrian's villa, and at this time I had no idea that cobalt was known to the ancients. Mr. Hatchett and Mr. Klaproth had both found oxide of copper in some ancient blue glasses, which I conceive must have been opaque.

was pale grass green, had the character of carbonate of copper mixed with chalk; and a third, which was sea green, was a green combination of copper mixed with the blue copper frit.

All the greens that I examined on the walls of the baths of Titus were combinations of copper. From the extreme brilliancy of a green which I found in the vineyard to which I have so often referred, I suspected that it might contain arsenious acid, and be analogous to Scheele's green; but on submitting it to experiments, it afforded no indications of this substance, and proved to be a pure carbonate of copper.

The greens of copper were well known to the Greeks; the most esteemed is described by Theophrastus and Dioscorides under the name of *χρυσοκόλλα*, and is stated by both to be found in metallic veins.

Vitruvius mentions *chrysocolla* as a native substance found in copper mines, and Pliny speaks of an artificial chrysocolla made from the clay found in the neighbourhood of metallic veins, which clay was most probably impregnated with copper. He describes it as rendered green by the herb luteum. There is every reason to believe, that the native chrysocolla was carbonate of copper, and that the artificial was clay impregnated with sulphate of copper rendered green by a yellow dye.

Some commentators have supposed that chrysocolla is the same substance as borax, because Pliny has mentioned that a preparation called by this name was used by goldsmiths for soldering gold*; but nothing can be more gross than this mistake, which, however, has been copied into many elementary books of chemistry. The material used for soldering gold consisted of carbonate or oxide of copper mixed with alkaline phosphates. This is evident from the description of Dioscorides "*Περὶ ἰοῦ σκώληκος*," lib. v. c. 92, who says it was prepared from urine treated in brass mortars. Pliny says likewise, that it was prepared from "Cypria ærugine et pueri impubis urina, addito nitro†." The name of chrysocolla was probably derived from the green powder used by the goldsmiths, and which contained carbonate of copper as one of its ingredients‡.

Amongst the substances found in the baths of Titus were some masses of a grass green colour. I at first thought these might

* Hist. de la Peinture ancienne, pag. 38. "Nos droguistes la nomme Borax." † Lib. xxxiii. cap. 5.

‡ The commentators have been likewise misled by Pliny's description, "chrysocolla humor est in puteis per venam auri defluens, &c." Ibid.; but this is merely an inaccurate account of the decomposition of a vein containing copper. We have no reason for supposing that the Greeks and Romans were acquainted with borax. Pliny, probably misled by the application of the same name to different substances, considered chrysocolla as the cement of gold in mineral veins.

be

be specimens of native chrysocolla; they proved indeed to be carbonate of copper, but it had formed round longitudinal nuclei of red oxide of copper, so that probably these substances had been copper nails or small pieces of copper used in the building, converted by the action of the air, during so many centuries, into oxide and carbonate.

The ancients, as it appears from Theophrastus, were well acquainted with verdigrise. Vitruvius mentions it amongst pigments; and probably many of the ancient greens, which are now carbonate of copper, were originally laid on in the state of acetite.

The ancients had beautiful deep green glasses, which I find are tinged with oxide of copper; but it does not appear that they used these glasses in a state of powder as pigments.

The greens of the Aldobrandini picture are all of copper, as was evident from the action of the muriatic acid upon them.

[To be continued.]

LXIV. *The Electric Column considered as a maintaining Power, or First Mover for mechanical Purposes. By* GEORGE JOHN SINGER, *Esq.*

THE power of the electric column as a source of mechanical action, was first discovered and applied by that excellent philosopher M. De Luc, the admirable inventor of that important instrument; and it is to his active discrimination and unceasing exertions we are indebted for the principal mechanical arrangements which have been employed to render the variable action of the column equal to the production of a constant though unequal motion.

The principal object of such an attempt is to enable an observer to measure the actual variation in the power of the column at different times, and under dissimilar circumstances; and, by a comparison of these changes with the usual meteorological phænomena, to ascertain if any connexion can be traced between the spontaneous electricity of the column, and the natural electricity of the earth and the atmosphere.

For this purpose any arrangement may be employed which is capable either of producing or maintaining the motion of light substances by the immediate action of the column; and that will be most eligible which produces this effect most certainly, and by the least complex means.

With columns of small power, the frequency with which the leaves of Bennet's electrometer are made to open, and strike the

 sides

sides of the glass, during their contact with one extremity of the column, for a certain number of seconds, becomes a measure of the comparative power of the instrument at different times: but its distinct expression is prevented by the tendency of the gold-leaves to stick to the sides of the glass; and this arrangement is therefore by no means fitted for permanent observations.

When an insulated conducting substance is freely suspended between two balls, or bells, connected respectively with the opposite ends of the column, I have found that motion is constantly produced, if the weight of the pendulum, and the distance of the bells, are *exactly* proportioned to the acting power of the column at its *mean* rate of intensity: but if these circumstances are not strictly attended to, the motion will soon cease; and the want of complete success in the original experiments of M. De Luc and of Mr. B. M. Forster most probably arose from this cause; for, in the construction of a number of instruments on this plan, I have had but one failure, and in that instance the apparatus was finished in such haste as to preclude a proper attention to the circumstances above stated.

Fig. 1. Plate VII, represents the arrangement of my Electric Chime. A series of about 1600 groups of zinc, silver, and paper disks, are disposed in two columns, separately insulated in a vertical position; the positive end of one column is placed lowest, and the negative end of the other, their upper extremities being connected so as to form in effect one series, having at each of its extremities a small bell; between the bells a small ball is suspended by a thread of raw silk, so as to hang at an equal and very small distance from each of them if unelectrified. The action of the column occasions this ball to vibrate between the bells and produce an electric chime, in which the variable action of the instrument at different times is indicated by an increased or diminished velocity of ringing. There is a circular groove in the base of the instrument which receives the rim of a glass shade, by which dust and moisture are prevented from impeding its action.

Fig. 2 represents a convenient modification of the arrangement devised by M. De Luc, and to which he has given the name of *Aërial Electroscope.* It is constructed nearly in the same manner as the chime, but has balls at its lower extremity instead of bells. From the positive end a wire W proceeds upwards a few inches parallel to the column, and is then bent into a hook to serve as a support to the pendulum, which consists of a fine silver wire to which a gilt pith-ball is attached. This

* This Plate will be given with the next number.

pendulum,

pendulum, being in conducting communication with the positive extremity of the column, will necessarily recede from it and approach the opposite ball; but it is prevented from actual contact with that ball by a brass fork F, across which a very fine silver wire is stretched. This wire discharges the electricity of the pendulum, and at the same time produces a kind of jerk which prevents the pith-ball from sticking: the pendulum now falls again into contact with the positive ball, but becoming again electrical recedes from it and again strikes the cross wire; and in this way, if properly constructed, may continue its vibrations for an unlimited period.

I have sometimes made a variation in this apparatus, by removing the cross wire and the conducting support of the pendulum, and by substituting for it a pith-ball suspended by a silk thread, and accurately proportioned in weight and size to the medium power of the column. By this means the motion occurs over more space than in either of the preceding arrangements, and is therefore more obvious, and well calculated for observation, as the irregularity is considerable, and may be noticed when the temperature of the surrounding medium varies but slightly.

During my employment of the very extensive series of columns I have constructed, I have frequently attempted to produce a rotatory motion by the direct action of their electrical power, but hitherto the attempt has continued unsuccessful; by indirect means, however, the same object has of late been very ingeniously obtained. In October last, my friend Mr. Lightfoot, a very active philosopher, who has made many interesting observations on this subject, first suggested the employment of an inflexible pendulum as a means of converting the reciprocating motion usually produced by the column into a source of rotatory movement; and the correctness of this idea was soon afterwards practically verified by my pupil Mr. F. Ronalds, who with the assistance of a watchmaker has made a very successful and truly ingenious arrangement, by which a simple and curious electrical clock is produced.

The rotatory motion obtained by this indirect means, is however rather curious than useful; for it is scarcely so correct an indication of the power of the column as the simple pendulum, and requires a much more extensive series to keep it in motion; it cannot therefore be preferred for the usual purposes of observation, and has I fear very little chance of becoming at all useful as a time-keeper; for the variable action of the column must render it a most irregular maintaining power, which it will be very difficult, if not impossible, to correct effectually.

The most elegant and at the same time the most simple movement yet produced by the action of the electric column appears to

to be that employed by Signior Zamboni, who has made some interesting discoveries on the general structure of the instrument. He employs a vertical needle supported by a delicate pivot or knife-edge a little above its centre of gravity, the position of which may be readily altered by means of a sliding weight attached to the lower extremity of the needle, which may by that means be so adjusted as to possess the properties of an accurate scale-beam, and will maintain its oscillations over a considerable space by a very slight impulse.

The upper end of the needle, for at least an inch, is formed of varnished glass; and on this a ring of gold, or a gilded ball of pith or cork, is fixed; the axis of the needle is supported midway between two vertical columns insulated, but connected together at bottom, so that the upper ends of the columns become the positive and negative extremities of the series; the upper and insulated extremity of the needle comes in contact alternately with each of these ends, receives its electrical state, and recedes towards the other, where the same process ensues; and thus the vibrations of the needle are maintained with great constancy over a considerable space.

Fig. 3 represents the form I have employed for this construction: the needle is supported by a brass arm which slides on one of the columns; it is suspended by a delicate pivot, and has at its summit a fine varnished glass tube to which a gilded ball is affixed; the lower extremity of the needle is provided with a sliding weight, by which the relation of the centre of gravity to the point of suspension is accurately adjusted: to render the contacts perfect, and least liable to change, the gilded ball does not strike the brass caps of the columns, but touches alternately two gold wires connected with them.

In this construction the needle is not moved by the direct attraction of the column; but being once put into a state of vibration, its motion, which would naturally decline, and finally terminate by the operation of friction and by the resistance of the air, is renewed at each contact by the impulse of electrical attraction, which is alternately exerted on the needle in opposite directions by each extremity of the column; and as this attraction does not sensibly act on the pendulum until it is very near the attracting surface, its operation commences when it is most wanted, and, without materially affecting the action of the pendulum in any other way, occasions it to describe constantly equal arcs at every vibration.

It is obvious, that by connecting a proper lever and ratchet-wheel with the axis of the needle, motion may be readily communicated to indexes, or to other wheels; and this I am informed has been done during the past year, by some experimentalists

talists on the continent. I have since tried the experiment, and find it succeeds perfectly, but requires a more extensive series to overcome the increased friction.

An effect very nearly resembling the action of the beam of a steam-engine may be produced by placing the needle in a horizontal instead of a vertical position. For this purpose it should be constructed in the same manner as an ordinary scale-beam; having equal arms terminated by gilt balls, and its point of suspension above its centre of gravity. If a needle of this kind be insulated, and placed with one of its balls a few inches above the positive extremity of a powerful column, whilst the opposite ball is similarly situated with respect to the negative extremity, it will, when once put into a state of oscillation, continue to move with considerable regularity, and with a momentum which renders it probable that, by the application of a proper mechanical arrangement, a tolerably regular source of rotatory motion would be obtained.

I have now completed a series of columns comprising upwards of 50,000 groups of a peculiar and powerful arrangement, but have not as yet combined them so as to institute any accurate experiments on their effects; but I trust it will not be long ere I have leisure to accomplish this object.

London, May 1, 1815. G. J. Singer.

LXV. *On the Coal and Stone Strata of Durham.* By Nat. John Winch, *Esq. of Newcastle.*

To Mr. Tilloch.

Sir,—If your correspondent who writes under the signature of "A Constant Reader," in the Philosophical Magazine, has access to the papers of the Geological Society of London, I beg leave to recommend him to inspect the numerous sections of collieries situated in this vicinity, which are deposited in the library of that Institute. By taking this trouble, he will soon perceive that strata of stone and coal vary not only in thickness, but are sometimes replaced by beds of different descriptions at places by no means distant from each other; and of course that a single section, such as that adopted by Mr. Forster, can convey but a very inadequate idea of the stratification of this part of the kingdom. But should his residence be out of town, the following information relative to the Newcastle grindstone bed may prove acceptable to him. On Gateshead Fell and Wickham Banks (see the Map of Durham) this stratum is 11 fathoms thick, and lies 38 fathoms above the High Main Coal; it is also met

met with at Byket Hill (see the Map of Northumberland), but is not found in the vale of Team which divides the former, nor of Tyne which separates the latter places. At Byket St. Anthon's Colliery (see Forster's Section), and in the Dean near Bill Quay, its thickness is 12 fathoms; and its place 58 fathoms above the High Main Coal; for it is worthy of remark, that the strata in this coal-field thicken on approaching the point of extreme depth situated near Jarrow. The stone quarried and sunk through at Byket Hill and Byket St. Anthon's is composed of much coarser grains than on Gateshead Fell, and by the colliery sinkers is called Brown post.

Respecting the muscle shells—these organic remains occur in most places where ironstone is found in the shales; not only in the Newcastle coal formation, but also in the metalliferous limestone district upon which it rests, where encrinal limestone, sandstone, and shale alternate; but the localities are too numerous to mention.

Very respectfully,

Your obedient humble servant,

Newcastle-upon-Tyne, May 8, 1815. NAT. JOHN WINCH.

LXVI. *Account of a recent melancholy Occurrence at Heaton Colliery.* *By* A CORRESPONDENT.

To Mr. Tilloch,

Newcastle-upon-Tyne, May 14, 1815.

SIR,—THOUGH some of the London prints have published accounts of the dreadful accident which happened at Heaton Colliery, on Wednesday the 3d of this month; yet as these paragraphs appear to be only short extracts from our provincial newspapers, the editors of which have omitted a description of the local situation of the mine and other circumstances well known, and of course uninteresting to their readers in this part of the kingdom;—I trust the particulars now transmitted you will be deemed worthy a place in the Philosophical Magazine; and under the impression that your Journal is perused by many gentlemen connected with mining pursuits, I shall offer no apology for detailing some minute occurrences, or occasionally making use of technical terms when applicable.—Heaton Colliery lies a mile and a half east of this town, and was won about twenty-five years ago; the workings are carried on in the High Main Coal, from the dip where the engine-pits are sunk to the depth of about eighty fathoms, towards the rise, where there may be twenty-five fathoms covering on the seam: but here the present

present owners have put down no shafts; the inclination of the strata is nearly east and by south. To the west and north-west of Heaton Hall are the wastes of ancient collieries long since abandoned. The conductors of the mine, well aware of the great body of water they would have to contend with when they approached these boundaries, had increased the number of their steam-engines to three (one of which is of the power of 130 horses), the whole being capable of drawing 1200 gallons each minute; and by this prudent precaution had already freed themselves from the water pressing upon them from the west; but that accumulated towards the north-west was still to be let off. For this purpose a drift was driven in a direction to perforate the old workings near Heaton Burn, a little to the north and by east of the Hall, where the remains of numerous pit heaps are to be seen. An upcast dike of eight fathoms had just been passed through; this should have served to point out the spot where the miners of former times were from necessity likely to suspend their labours; and now all due precaution should have been used to guard against impending danger. This drift in the stone had been driven in a sloping direction upwards to regain the seam, and two feet of coal already formed its roof.—At four o'clock on the fatal morning, Mr. Miller, the resident or under viewer, visited the men engaged in this operation, and a dripping of water from the roof being pointed out to him, he gave directions that the *work should be squared up;* and said "he would send in the borers at nine o'clock with the next shift, to ascertain whether the water proceeded from the wáste or no. In less than a quarter of an hour after this conversation took place, the water began to run more freely through the chink; and the two drifters becoming rather alarmed, sent their boy to apprize two other men who were working near them, with the state of the mine; also directed him to go to the crain where the caves of coals are delivered from the board-ways into the waggon or rollyway, and acquaint the whole of the men in the pit of their danger. The youth asserts that no one was at the crain when he reached it; but this is altogether improbable, and no doubt is entertained that, impelled by fear, he made the best of his way to the shaft and escaped. The two workmen first mentioned had now quitted the face of the drift, and one of them recollecting that he had left his jacket behind, proposed to the other to return and see how the *water was coming off;* but at this instant a frightful crash, accompanied by a violent gust of wind, which extinguished the candles, warned them that an immense torrent of water was rushing into the mine; they fled precipitately towards the working shaft, distant about a mile; and, as the water of course flowed first down the lowest level, reached

it

it just in time to save their lives. The two men who were working near them, the boy before mentioned, and fifteen other men and boys who were on the rollyway, were so fortunate as to make their escape, but not till the last of them was up to his waist in water. Every possibility of retreat to those left behind was now cut off; and, shocking to relate, seventy-five human beings, including Mr. Miller, were shut up in the workings towards the rise of the colliery, either to perish by hunger, or die for want of respirable air; and twenty-four hours elapsed before the water rose to twenty-five fathoms in the engine pit; if it could even then displace the air confined in the higher part of the mine. Some faint expectations were entertained during the course of the first day, that a communication might be opened to these unhappy people, by uncovering and descending through one of the old shafts at Heaton Banks; but before the scaffolding was reached, the surrounding earth fell in, and every glimpse of hope vanished.—Workmen are now busied in clearing out a shaft in front of Heaton Hall; but owing to rubbish which has accumulated at the bottom, and the carburetted hydrogen which is ascending in great profusion, much time must be spent before that part of the workings can be inspected, where most of the pitmen were known to be employed when the accident took place.

The sufferers who thus found a living grave have left twenty-four widows and seventy-seven orphans, besides Mrs. Miller and her eight children, to deplore their untimely fate.—I am happy to add, that a subscription is now on foot to relieve the pecuniary distress of their families. N.

P. S.—Thirty-seven horses were in the mine at the time of the catastrophe.

LXVII. *Notices respecting New Books.*

Observations, chiefly practical, on some of the more common Diseases of the Horse; together with Remarks upon the general Articles of Diet, and the ordinary Stable Management of that Animal. By THOMAS PEALL, *Veterinary Professor and Lecturer to the Right Hon. the Dublin Society, and Veterinary Surgeon in the Royal Artillery.* 4to, 360 pages.

MR. PEALL speaks with much modesty of his pretensions as an author. His work, however, is replete with so much useful practical information, that it cannot fail to prove acceptable, not only to professional people, but to every gentleman who would rescue his cattle from the destructive empiricism of grooms and common

common farriers. The author's views of some of the diseases to which this noble animal is liable, appear to us to be new, and the treatment recommended, highly judicious. He ridicules the idea of a vitiated, foul state of the blood and humours being the cause of *the grease*, the great winter disease of horses that are kept in large crowded towns. It is seen but rarely (in comparison) in the country, especially among such as are employed in agriculture; and least of all among those of the latter kind that perform work of a regular kind. No disease to which the horse is liable, appears to the author to arise more decidedly from the ordinary management of stables, or to be less a *necessary consequence* of domesticating this animal. In opposition to the opinion of the late professor St. Bel, that this disease is contagious, he urges, that where numbers of horses are all treated in the same way, and exposed to the same exciting cause of disease, nothing can be more natural than that numbers should be attacked with similar morbid symptoms—"Stripped of all mystery and all the technical language of the schools; the grease may be considered, simply, as an inflammation of the skin of the fetlock joint. The disease is, in truth, the chilblain of the horse." ......"We shall cease to wonder that the skin of the fetlock is liable to be attacked with inflammation, especially in the winter season, if we consider the peculiar circumstances to which it is exposed......The seat of the disease is remote from the great fountain of life, the heart......The fetlocks are exposed to greater vicissitudes of heat and cold than any other part of the animal—at one moment enveloped in a bed of hot litter and fæces at a temperature of 50° to 60°; the next, exposed to a current of cold air several degrees below the freezing point;"—and sometimes standing for hours in snow, or bruised ice. "The skin is frequently not merely imbued with moisture, but with such as is of a most deleterious kind, to a part susceptible of inflammation, namely, the urine of the animal; which contains a great deal of volatile alkali, even before that salt can be engendered by the putrefactive fermentation of the litter." We regret that our limits do not permit us to lay before our readers the whole of the author's reasoning and proofs in support of his opinion. Though the author considers the grease not as a constitutional but as a local disease, brought on by external causes alone, yet he does not maintain that internal remedies ought *never* to be used—"for it happens, every now and then, that the inflammation of the diseased limb is so prodigiously high, that the constitution is found to sympathize with the diseased parts, and generally fever and derangement of the system are the consequences. But the chief reason why this complaint is frequently found so difficult of cure, is owing to the circumstance of considering

dering it a primary disease of the constitution; which, it is supposed, must be amended before the complaint can be cured. Hence the farrago of alteratives," &c. &c. which are frequently exhibited for weeks, sometimes months, where a judicious application of simple external remedies would be found capable of effecting a cure in as many days. But we must refer our readers to the work itself for the author's mode of treatment. The contents of the work are:—On the Use and Abuse of Purgatives;—on Strangles;—on Sprains or Claps in the back Sinew;—on Worms;—Spasmodic Colic or Gripes;—Inflammation of the Eyes;—Inflammation of the Lungs;—the Grease;—the Farcy;—the Glanders;—General Treatment of the Feet;—Inflammation of the Bowels;—Stables;—Clothing;—Litter;—Water;—Light;—Grooming;—Exercise;—Food;—Formulæ of Medicines.

Travels in South Africa, undertaken at the Request of the Missionary Society. By JOHN CAMPBELL. 8vo. 596 pages.

We notice this publication (in which there are some curious details, which cannot but prove interesting to those particularly who recommended the voyage) chiefly for the purpose of giving a few extracts of detached facts which may be acceptable to such of our readers as are fond of natural history. The author's route, marked on a map which is given in the volume, was from Cape Town, about 18° 20′ S. long. and 34° S. lat. in a direction nearly east to 27° 10′; then northerly (with some westerly deviations) to lat. 27° 40′ S. whence he proceeded easterly, then SW. till he reached the banks of Yellow River, at about 26° 30′ E. long. and 20° 30′ S. lat. This river runs westerly, and towards its mouth takes the name of Great or Orange River. It runs into the South Atlantic Ocean at about 16° 50′ E. long. and 28° 30′ S. lat. Mr. Campbell's general route was then in the direction of this river, from the point at which he approached it to about 18° E. long., when he travelled southerly, the most direct way he could find to Cape Town. In this route, a great part of which had never been visited by any European, the author encountered the usual inconveniences which accompany travellers among uncivilised tribes, and met with some facts so curious as to procure them a slight notice in his pages, though his journey had other and more important objects in view—the future civilisation and moral improvement of the sons of Africa. Mr. Campbell left England in July 1812, and reached Cape Town on the 24th of October. After a few weeks residence there, and visiting different missionary settlements, the author and his small party set off on his projected journey on the 13th of Feb. 1813, with three waggons (afterwards augmented to five) each drawn by

twelve

twelve oxen, and did not get back to the Cape till the end of October.

Feb. 16.—Killed a gray serpent, which shone in the dark, and emitted a rattling sound, evidently intended by Providence to give warning of its approach.

March 15.—Counted 29 aloes in flower. Some of the stalks measured 38 feet in height, and $2\frac{1}{2}$ feet in circumference at the bottom;—a wonderful growth in one year! What a curiosity would these be esteemed in the vicinity of London, where it is believed they come into flower only once in 100 years! If an aloe produces seed when it sends on a flower, it dies that year; if not, it lives and sends forth a flower again.

March 19.—Saw numerous ants' nests. In shape they resemble a baker's oven, and are from two to four feet high. These industrious animals have their enemies, especially a creature about the size of a fox, who, after piercing a hole in the side of the nest, pushes in his tongue, when the unwary ants rush towards it in order to investigate what has happened. The tongue being covered with these insects, he draws it in, and swallows the whole, repeating the process till he has devoured millions. The bees sometimes covet and take possession of the house. The boors also, when travelling, frequently clear out these nests, and use them as ovens for baking their bread.

April 13.—Came in sight of some springbucks, which afforded great amusement, from their springing at least six feet every leap in height, and several yards in length. However near a person may be to them, no motion of their legs can be perceived. The instant they touch the ground they rise again into the air, which makes their motion resemble flying. When they leap, the ground on which they light seems as if elastic.

April 25.—A honey-bird, which leads travellers to hives of bees, invited us, by chirping, to a place where honey might be found. One or two of our escort went towards it, when it flew from tree to tree chirping; but as the hive appeared too distant they returned.

May 18.—Commenced keeping watch at night. The Hottentots watched chiefly on the lee side of the waggons: the reason I understood to be, that a lion or a Bushman never makes an attack from the windward, because then the dogs smell them and give an alarm.

May 21.—This season may be called the Bushmans' harvest. The ground being now softened by rain, they pull up roots, not only for present consumption, but for future use. In summer they are supplied with locusts, which they dry and pound into powder, which serves as a substitute for flour.

June 7.—Plagued with bushes significantly named *stop-a-while.*

while. Its thorns exactly resemble fishing-hooks; so that, if they catch hold of your clothes as you pass, you *must* stop, sometimes a long while, before you get clear of them. In clearing one arm the other is caught; and without the cautious assistance of a second person there is no escaping, but by main force and losing part of your clothes.

June 17.—Halted at Blink (or Shining) Hill, so called on account of a shining stone, resembling the lead of which pencils are made in England. This the Bushmans and others grind to dust, which they use as we do hair-powder. A red stone with which the surrounding nations paint their bodies, comes also from this hill, and forms an article of barter. This mountain is a kind of Mecca to the surrounding nations. They are constantly making pilgrimages hither, not indeed to pay religious homage, but to obtain supplies of blue powder for their hair. This custom has existed time immemorial. Accompanied by Mr. Reid and our Hottentots with candles, the mine was entered. After descending with some difficulty, we soon lost sight of the world, sometimes wading half-leg deep in black-lead dust. The roof was full of projecting pieces of the shining rock, and large caverns appeared on each side as we advanced. The roof at one place appeared curiously carved. Touching a part of it which we could reach, we perceived it had life. It proved to be composed of a multitude of bats hanging asleep from the roof. Moving them backwards and forwards neither awoke nor made any of them lose their hold of the rock, to which they clung by the claws of their hinder legs; but holding the candle at a little distance under one of them awoke it, when it flew to another part of the cave. Having advanced about 100 feet, the cavern became so low and narrow, that we could proceed no further in that direction. We returned, and went, by a passage leading to the right, deeper into the mountain, and entered a large cavern the bottom of which was strewed with the bones of animals, and some parts indicated fires having been made in it. After collecting some specimens of the rock and powder, we returned to day-light nearly as black as chimney-sweepers.

Killed a buffalo and wounded two. The buffalo is often extremely furious when wounded and not disabled. Should his enemy climb a tree, he is far from being out of danger; for the animal will run with violence and strike the tree with his massy horns, which cover as with a helmet the crown of his head; the stroke of which will so shake the largest trees as to require a firm hold indeed to prevent the person falling to the ground, and being consequently tossed by the horns of the enraged animal. This buffalo was food for ourselves and followers for several days, saving the sheep for future use.

June

June 24.—The travellers reached Lattakoo, a royal African city. By the map it appears to be situated in long. 25° 55′ E. lat. 26° 20′ S. where they remained till the 7th of July. They were treated with hospitality: not a single article was stolen from them during their stay, except *two buttons*, for which the culprit was driven out of the public square. The king agreed to receive missionaries for the instruction of his people. They had heard, they said, that there was *a Great Being*, but they knew him not, having never seen him. On one occasion, about a dozen people sitting a few yards from Mr. Campbell's tent were singing one word with a little variation, viz. *Hailylallay—Hielaylallay*. After repeating it six or eight times in chorus, they paused, and began all at once again. [May not this be a corrupted pronunciation of *Hallelujah?* which, with some varieties in pronunciation, is still in use among all the northern inhabitants of Africa.]

Aug. 11.—Hardcastle is surrounded by mountains of asbestos. It is disposed between rock strata. That which becomes, by a little beating, soft as cotton, is of a prussian blue colour. I found some of the colour of gold, but not soft or of a cotton texture like the blue; some I found white, brown, green, &c. Cloth of this substance stands the fire. The ancients burnt their dead in such cloth, to obtain their ashes; and it is remarkable that in the language of the Griquaas it is called *handkerchief stone*.

Aug. 14.—Observed a camel-thorn tree stripped of its leaves by the winter, which was just over; but there were three branches of a different kind of tree, or bush, which had been engrafted into it by a bird; which is a common occurrence in this country: these branches were full of leaves.

Sept. 10.—A tree was observed having two remarkable nests on it; one about four yards in circumference; the other three, and about a yard in depth. They are built of coarse grass, by a small bird resembling our goldfinch. One of the nests had seventeen holes in the bottom, the other seven, by which the birds enter. At one time I saw about a hundred come out of them. Instead of being the nest of a single pair, they seem to be kraals or towns of birds.

I was for some time surprised how such multitudes of lizards and mice as inhabit the Desert could live without water; but I observed many succulent plants of various kinds loaded with small berries containing water. I poured out of one large berry about three teaspoonfuls of water; and I witnessed the mice dragging them to their holes, just as seamen take casks of water into their ships. This is a wonderful provision God has made to supply the wants of these little animals.

 A stone

A stone having been thrown at a lizard, which struck off its tail; the tail leaped about for five minutes, and moved, something like a serpent, to three yards distant. The poor lizard as well as myself stood wondering at this exhibition. The Hottentots informed me that, like serpents, after their heads are cut off they live until the going down of the sun.

The natives have a strange way of getting rid of flies. They rub milk over their sheep, and, placing them before the door, drive out the flies: these instantly light upon the sheep, which are then driven to a distance.

Sept. 28.—Four scorpions were put into a hole dug in the earth. They soon began to fight. The mode was curious. Having two claws like a crab's, with these they attempted to seize each other by the head. When one happened to be thus caught, he seemed sensible of his danger and cried out; but the other, regardless of his cries, turned round his tail and gave him one sting. The one that was stung, as if aware of its mortality, resisted no more, but lay down till he died. The other, as if aware of the same thing, gave himself no further trouble. They all had the same method of fighting, and all the vanquished acted in the same manner. The surviving conqueror was put to death, as too dangerous to be allowed to escape.

April 21.—On the voyage home crossed the equator, and had an opportunity to see verified Dr. Franklin's assertion, that oil thrown upon agitated water will smooth it. A South Sea whaler near us pumped out her bilge-water, which was mixed with oil; when the sea for a quarter of a mile behind her became as smooth as glass.

A practical Treatise on Gas Light; exhibiting a summary Description of the Apparatus and Machinery best calculated for illuminating Streets, Houses, and Manufactories with carburetted Hydrogen or Coal Gas; with Remarks on the Utility, Safety and general Nature of this new Branch of civil Œconomy. By FREDRICK ACCUM. 8vo. Ackerman. pp. 200.

Our readers are of course aware that the rapid increase in price of late years, of the materials for producing light, has given rise to a new and beautiful substitute for oil, wax, and tallow; this new method consists in burning the gas obtainable from common pit-coal by distillation, and the light so obtained is known by the name of Gas Light.

At an early period in the history of the introduction of this improvement into the British empire, Mr. Accum was called upon to give the aid of his extensive practical knowledge as a chemist, in investigating the merits of the new institution, and had

had the honour to be selected to give evidence of its utility and œconomy before both houses of parliament. It is with peculiar satisfaction, therefore, that we find him coming before the public as the author of a popular treatise on this new branch of domestic œconomy, convinced as we are that few could be found better qualified for the task.

The more immediate object of Mr. Accum's treatise is to describe the apparatus now so successfully employed for manufacturing coal-gas in the large way, and to point out the means adopted for carrying this new mode of illumination into effect, for illuminating streets, houses, and manufactories.

The work is divided into two parts: the first part is in some measure preliminary, and independent of the second. The former embraces a concise and popular view of the general theory and production of artificial light—the illuminating powers of candles, lamps, and gas lights of different kinds, with regard to the quantity of combustible material consumed; the mode of measuring the intensities of light obtained from different combustible bodies, so as to ascertain their value; together with such other facts and observations as the author deemed necessary to clear the road, and enable those who are not familiar with chemistry fully to understand the nature of the gas-light illumination.

This is succeeded by a chemical view of the general nature of coal—the chemical changes which this substance undergoes when employed in the production of gas-light—the different products it furnishes—the operations necessary for obtaining them—their quantities and applications in the various arts of life—together with an historical sketch of the rise and progress of the gas-light illumination.

The second part of the treatise, and by far the most copious, is dedicated to the summary details of the new art of illumination. We there find the data for calculating the expense that must attend the application of this new species of light, under different circumstances, when compared with the lights now in use—the obvious effects which the discovery of lighting with coal gas must inevitably produce upon the arts and upon domestic œconomy—its primary advantages—its ulterior promises—its limits—and the resources it presents to industry and public œconomy. The author has candidly shown how far the gas-light application is safe, and in what respect it is entitled to the public approbation and national encouragement.

Our limits will not allow us to speak in detail of the merits of this work: the abilities of the author have been long known as a philosophical and practical chemist. In the present work he

has displayed much candour and impartiality, and knowledge of the subject on which he writes. His book will be found highly useful to those who wish to acquire a practical knowledge of the subject on which it treats, and will enable mechanics to erect the apparatus necessary for carrying the gas-light illumination into effect. It will remove many misapprehensions concerning the safety of the new lights; it will show the impossibility of streets or towns illuminated with coal being suddenly thrown into darkness by the fracture of one or more gas-light *mains*, or by the destruction of one or more of the gas-light machineries, as has been represented by some misguided individuals; and it will give to those who are unacquainted with the nature of the gas-light illumination, a fair and not over-charged statement of the merits and defects of this new art of illumination; whilst at the same time the chemist will meet with facts relating to the subject of the gas-light illumination, which will arrest his attention, and add to the general stock of chemical knowledge.

The plates, which are seven in number, are neatly engraved: they exhibit not only the large machineries now successfully employed in lighting streets, houses, and public edifices in this metropolis, but likewise the smaller apparatus made use of in manufactories and other private establishments.

Before concluding, we cannot avoid recommending to the different gas companies to pay attention to the purifying of their gas by passing it through water, and particularly to avoid the use of coals containing sulphur, as we have noticed that wherever sulphur is burnt, as in vitriol manufactories, the workmen always lose their teeth, and very speedily too. We mention this with no invidious intention; for we consider the introduction of gas-light into general use as an object of great national importance; and we would be sorry, should ignorance or inattention on the part of those who embark in such undertakings, as objects of commercial speculation, induce an effect which would set the whole community in array against this mode of illumination.

LXVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

April 29. A PAPER was read from Mr. Seppings, containing some additional remarks to his former one, on an improved method of ship-building. The author chiefly directed his attention to some points commented on by Dr. Young, and alleged the insufficiency of mere theory when confronted by practice: he dwelt particularly on the fact, that the French never built any ships

ships with diagonal timbers, and afterwards abandoned the mode as useless. The utmost, he maintained, that the French had done, was building one ship the lining of which was placed diagonally; but this, as might be expected, was attended with none of the advantages which his method possessed, and on the contrary had several peculiar disadvantages. Mr. S. also produced various certificates from different naval surveyors, and others, all of whom concurred in proving that the ships built according to the plan which he had carried into execution were almost entirely exempt from hogging or becoming broken-backed.

May 4. Sir Humphry Davy, having happily returned from his scientific tour on the continent, communicated an interesting paper on the hyperoxymuriates, and on what Gay Lussac calls chloric acid.

Dr. Phillip, in a short paper, stated his having found a fœtus in the abdomen of a child. At the age of two years and a half a hard tumour was observed in a child's abdomen; various means were taken to reduce or discuss it, but in vain; all medicine was ineffectual; and when the child died, its body was opened, and what had been supposed a tumour, proved to be a fœtus. Dr. P. thinks this an instance of one fœtus being inclosed within another in the womb.

May 11. Mr. Poret junior related a series of very delicate and complex experiments, in addition to his former paper, which appears in the Phil. Transactions on the salts called triple prussiates. Notwithstanding the apparent accuracy, however, of his experiments, he found them not quite correspondent with the atomic theory, and therefore rejected the inferences to be drawn from them, or the facts which they developed, preferring rather to believe in the infallibility of his calculations of atoms, than in the correctness of his actual experiments. The Society then adjourned over one week.

May 25. A paper by Dr. Parry was read, on the nature and cause of the pulse. Dr. P. took a review of the different theories which have been proposed to account for the phænomenon of pulsation, observing that the greater part of physiologists had contented themselves with the opinion of Haller, that pulsation was occasioned by the diastole and systole of the heart. His view, however, of the question is much simpler: on examining different arteries where they were exposed to no obstruction or pressure, he found that they had no pulse: by pressing the finger on an artery over a soft part of the body, which yielded sufficiently to the pressure, no pulse was manifested; but whenever an artery was pressed over a solid part, then a pulse was immediately found. He repeated these operations several times, and uniformly found the same effects. Hence he concludes that

 the

the pulse is nothing more than the re-action or impetus of the blood to maintain its regular motion. The arteries appear only as canals through which the blood flows in a uniform and continuous current; diminish the diameter of these canals, and a pulse is immediately perceived. At every junction of a vein with an artery, the internal diameter of the latter is diminished, and hence a pulse always appears. This Dr. P. thinks fully adequate to account for all the modifications of the pulse.

Part of a very elaborate paper was read, detailing numerous experiments on malic acid, analysis of several vegetables which contain this acid, and on the crystallization of malat of lead. Gooseberries, and many other culinary vegetables contain considerable quantities of malic acid; but on discovering that malat of lead sometimes formed crystals, and at others continued in a thick mucilaginous state, the author was induced to vary his experiments to ascertain the cause of this apparent anomaly, when he discovered that it was owing to the presence of another acid. This acid also existing in vegetables when combined with the malic, the solutions of lead in it then assume a crystalline character. This fact is of great importance, as malat of lead has been deemed to be one of our most delicate tests or reagents.

ROYAL INSTITUTION.

Mr. Brande's ninth lecture embraced a general view of the doctrines of chemical affinity or attraction. This subject, although noticed and elucidated by Sir I. Newton, had been but sparingly investigated previously to the foundation of the French school of chemistry by Lavoisier and his associates: with them it became an object of early attention, and new ideas were formed upon it, which it now became needful to discuss.

The Professor observed, that at the outset of his present course of lectures, the distinctive objects and character of chemistry as a science had been explained, but that it would have been of little avail to have dwelt upon the *laws* of chemical changes; that about the present period of his history, the wavering and doubtful opinions entertained upon this head began to subside into steadiness and certainty, and the bickerings of unsupported hypothesis to vanish before the steady light of theories deduced from experiment and observation.

We were now also called upon to consider the improvements in the language of chemistry, and the grand nomenclatural reform which in 1787 was presented to the chemical world under the sanction of the united names of Lavoisier, Morveau, Berthollet, and Fourcroy.

Mr.

Mr. Brande proceeded to illustrate the effects and laws of chemical attraction, by an extended series of experiments: he observed, that a total change in the properties of bodies was the most obvious result of a chemical action; that insipid, inodorous, and insoluble substances acquired flavour, smell, and solubility; and that the third substance resulting from the union of two bodies, did not possess the intermediate qualities of its component parts, but exhibited new and unexpected properties; that change of form or state was a frequent attendant on chemical action; that solids became fluids and gases; fluids were solidified; and aëriform bodies, quitting their elastic state, acquired a liquid or solid form. These phænomena were separately illustrated by experiments.

Mr. Brande concluded his lecture with an examination of the laws of simple and double decomposition, and with a general view of the theory of definite proportions, in which he demonstrated the tendency of bodies to unite only in certain relative weights; and that, when they combine in more than one proportion, the second, third, or fourth quantities are multiples or divisions of the first. Mr. Brande contrasted these views with those advanced by Berthollet in his "Chemical Statics," and proved the correctness of the former, and the imperfection and futility of many of the latter.

In some experiments with carburetted hydrogen, the Professor took occasion to advert to the applications of this gas to œconomical purposes, which, with few inconveniencies, held out many advantages. The question was too extended and important to be then entered upon; he therefore merely noticed the quantity of gas afforded by a given quantity of coals, and the proportion required to feed an Argand's lamp. We understood that a chaldron of Newcastle Wallsend coals afforded 10,000 cubical feet of gas fit for burning; of which, between three and four cubical feet were consumed by each Argand's burner per hour.

This lecture was interspersed with several applications of the principles laid down, to the phænomena of art and nature.

It is obvious, said the Professor, that the effects of chemical combination are referable to simple laws productive of invariable results; and hence we discern, in the minutest atoms and combinations of matter, the same display of unerring harmony which is exhibited upon a great and magnificent scale throughout the system of the universe.

Mr. Brande in his eleventh lecture dwelt upon the advances which had been made in chemistry during the last century, and observed, that new and powerful instruments of research had been discovered. He then proceeded to discuss the relation of the mechanical forms to the chemical constitution of bodies; and,

and, having taken a general view of the phænomena of crystallization, noticed at considerable length the theories which had been invented to account for them.

It will be impossible to give an intelligible outline of that part of Mr. Brande's lecture in which he exposed the theories of crystallization: those of Romé de Lisle, of Haüy, and of Dr. Wollaston, were separately considered, and illustrated by a series of excellent models. The Professor supported that hypothesis which regards the primitive forms of crystals, as made up of simple arrangements of spherical particles; and some new facts upon the subject were adduced, which, if not perfectly demonstrative, were highly favourable to these views.

In his twelfth lecture Mr. Brande entered upon the subject of electricity, with an historical sketch of the rise and progress of that branch of experimental science. Theophrastus, the celebrated pupil and successor of Aristotle, is the first who alludes to this singular power of matter. Dr. Jelbert is the earliest scientific writer on this subject, and in his treatise on magnetism, published in 1600, many important facts concerning the excitation of electricity are detailed. Having detailed these and other discoveries in chronological order, the Professor proceeded to a series of experiments illustrating the laws of electrical excitation. The phænomena of *induced electricity*, and the other modes of excitation, were also considered.

Mr. Brande said that it was only within the last fourteen years that the chemist had been required to enter minutely upon this subject of inquiry; for, although it might have been suspected that an agent so powerful and so universal as electricity was not unconnected with the chemical energies of matter, no such relationship had been demonstrated until the commencement of the present century, an æra which would ever be referred to, as among the most glorious and important in experimental science.

Mr. Brande described the different *electrometers*, or instruments for ascertaining the presence and measuring the quantity of electrical excitation, and concluded with details respecting the discoveries of Dr. Franklin, especially as to the modes of preserving ships and buildings from the effects of lightning, and with some observations on his scientific character.

Mr. Brande in his thirteenth lecture resumed the subject of electricity. He dwelt upon the different modes of excitation shortly noticed in his former discourse, and referred the phænomena of the thunder-storm and water-spout, to induction taking place between the clouds and the earth and water. He illustrated the meaning of the terms *quantity* and *intensity*, as employed by electricians; the former signifying the absolute quantity of electricity diffused over any surface; the latter, its tendency to

fly

fly off or pass through non-conducting media: thus the electricity of the common electrical machine is possessed of great intensity, because it passes in long sparks through air; but its quantity is inconsiderable, for the whole is at once withdrawn from the conductor.

Mr. Brande next proceeded to the discoveries of Galvani and Volta: the former had ascertained that, on bringing zinc and silver in contact with the nervous and muscular parts of recently killed animals, violent spasmodic affections ensued; and he formed an unsatisfactory hypothesis to account for the effect: but the subject soon fell into the more able hands of Volta, and became productive of infinitely important consequences, not merely as influencing the theory of electrical action, but as extending the boundaries of that department of science, and ultimately of chemical philosophy.

Volta referred the effects upon the frog, to the excitation of electricity by the contact of the metals, and devised the method of accumulating the electricity thus excited, by the alternate arrangements of two different metals and moistened paper. The Professor having pointed out the improvements which had successively taken place in the Voltaic apparatus, stated that its action appeared to depend upon the disturbance of the electrical equilibrium by the two metals, rendering one positive and the other negative; and upon its increase by induction throughout the series.

The electrical phænomena of the Voltaic pile were further illustrated by the construction and effects of the electric column of Mr. de Luc, in which an active electrical arrangement is produced by alternations of dry paper, zinc, and silver. The consideration of the general powers and chemical effects of the Voltaic instrument Mr. Brande deferred to a future lecture, when he should employ the large apparatus, consisting of 2000 series, constructed in the Royal Institution under the direction of Sir H. Davy.

The lecture concluded with some general observations upon the advances which had been made in the theory of electrical action; a branch of science, in which, notwithstanding the talents and abilities displayed in its prosecution, there remained many recesses unexplored, and many fountains unexhausted.

Having in his former lecture described the structure and operation of the Voltaic instrument, Mr. Brande proceeded in his 14th lecture on Saturday, May 27th, to consider the methods of increasing the quantity and intensity of its electricity. He spoke first of chemical action, and exhibited several experiments, in which the power of ignition in the battery was increased proportionally to the action of the intervening fluid upon the

the plates: at the same time, however, the common electrical powers were diminished; for such a pile did not affect an electrometer more powerfully than when the cells were filled with pure water, or when paper was interposed between the plates, as in the column of De Luc.

The effect of increasing the number of alternations was exhibited by the large Voltaic apparatus constructed in the Royal Institution by Sir H. Davy. It produced a most brilliant arc of flame when two points of charcoal, connected with its extremes, were brought near each other; in vacuo the discharge was some inches in length, and exhibited a star and halo of blue light upon the negative point, and a bright spot and brush of light upon the positive point: these effects were particularly distinct and beautiful. The fusion and combustion of the metals, and the different conducting powers of fluids, were also shown by the help of this apparatus.

The increase in the *quantity* of electricity by increasing the size of the plates, was illustrated by a battery composed of twenty plates, each being two feet square. The largest apparatus of this kind had been constructed by Mr. Children: it consisted of twenty plates, each six feet wide, and two feet eight inches deep. Its powers of igniting metallic wires were of the most surprising description; but as an account of the experiments made by means of it was about to be laid before the Royal Society, Mr. Brande did not minutely describe them. A singular experiment was exhibited, in which a wire of silver and one of the same diameter of platina, united end to end, were made part of the circuit; the platina wire only became red hot: the effect was referred to the superior conducting power of silver to that of platina, the heating effect being greatest where most resistance is offered to the passage of electricity.

The Professor exhibited some other forms of the Voltaic apparatus, and concluded his Lecture with some observations on the relation of electricity to the phænomena of heat, and on its power of producing or passing into radiant matter, the more full discussion of which would be resumed in his next and concluding lecture.

ROYAL MEDICAL SOCIETY, EDINBURGH.

The Royal Medical Society propose as the subject of their Prize Essay for the year 1816 the following question:

"What changes of composition does the process of digestion in quadrupeds produce on earths, oxides, and earthy, alkaline, and metallic salts?"

A set of books, or a medal of five guineas value, will be given annually

annually to the author of the best dissertation on an experimental subject proposed by the Society; for which all the members, honorary, extraordinary, and ordinary, shall alone be invited as candidates.

The dissertations are to be written in English, French, or Latin, and to be delivered to the Secretary on or before the 1st of December of the succeeding year to that in which the subjects are proposed;—and the adjudication of the prize shall take place in the last week of February following.

To each dissertation shall be prefixed a motto; and this motto is to be written on the outside of a sealed packet containing the name and address of the author. No dissertation will be received with the author's name affixed; and all dissertations, except the successful one, will be returned, if desired, with the sealed packet unopened.

KIRWANIAN SOCIETY OF DUBLIN.

Wednesday, April 19. Part of a paper "On the Inadequacy of Galvanic Hypotheses," containing a discussion of the question "Whether the active principle in Galvanic phænomena be electricity," was read, by M. Donovan, Esq. Secretary.

The first subject of investigation was the muscular contractions produced in dead animals by the application of heterogeneous metals, a phænomenon which has been attributed to the restoration of the electric equilibrium between the metals. That the restoration of the equilibrium, is the cause, was questioned on account of experiments which tended to prove, that when means were used for restoring the equilibrium, and when this end must have been attained had there ever been a disturbance, contractions were excited by these metals as strongly as ever. A number of experiments were then adduced to prove that muscular contractions are never produced unless chemical action be going forward in some part of the apparatus; and a number of facts which seemed to oppose this view were shown to be of no force, on account of circumstances not hitherto attended to. These views were supported by numerous experiments on common, and what has been called Galvanic electricity, all of which appeared to harmonize with each other. It was even stated that common electricity does not affect dead animals, although passing through them in exceedingly large quantities, unless circumstances be present which render the operation of a chemical change probable: and this opinion was supported by trials upon various dead animals. Considerations were then stated, which were conceived to prove, that the cause of the sensation called the Galvanic shock cannot depend on electricity. Voltaic series were described

described which possessed no powers of any kind; yet it was shown that the component metals were of the most energetic kind, that the menstruum was very powerful with other metallic combinations, and that the want of energy did not depend on a balance of electro-motive power between the metals and menstruum. It was then shown that the plates of Volta, commonly urged as a proof that the evolution of electricity in Galvanic experiments does not depend on chemical action, afford no evidence, and that they have even no connexion with the question.

A review was then taken of the different Voltaic arrangements which have been discovered by philosophers, and it was shown that not one of them contradicts the statement that chemical action is the means by which Galvanic phænomena are produced: for, in every arrangementt hat possessed activity, chemical action of some kind was going forward.

The paper throughout the whole was illustrated by numerous experiments performed before the Society.

On Wednesday, May 17, the Society adjourned during the summer recess.

FRENCH INSTITUTE.

Department of Mathematics, drawn up by Chevalier Delambre, *perpetual Secretary.*

[Continued from p. 313.]

We have already, in our notice for last year, briefly analysed the memoir of M. Biot, on a new application of the theory of the oscillations of light, read to the Class at the latter end of 1813. The author there announces that he has extended to the substances of which the double refraction is the most energetic, such as arragonite and rhomboidal carbonated lime, the inquiries which he had only applied at first to substances the double refraction of which is so weak that the images of the luminous points, seen through glasses of parallel surface three or four centimetres thick, are not sensibly separated. He has found in this way, that in these crystals, as in all the others, the luminous molecules begin by oscillating around their centre of gravity to a certain depth, after which they acquire also a fixed polarization, which ranges their axes in two rectangular directions.

In order to observe these phænomena in any given crystal, we must attenuate its polarizing power, until the luminous molecules which pass through it make in its interior eight oscillations less. We attain this either by forming with the given crystal laminæ sufficiently thin, or by inclining them on an incident polarized ray, so as to diminish the angle which the refracted ray forms with the axis of double refraction; or, finally, (and which

which is most convenient,) by employing those two methods at once.

We shall also attain the same object, by transmitting in the first place this incident ray through a piece of sulphated lime of a proper thickness, the axis of which forms an angle of 45°, with the primitive plan of polarization; for, when a ray is thus prepared in order that it may be resolved into coloured fasciculi, it is no longer necessary that the polarizing form of the second plate should be very feeble: it is sufficient for it to combat and weaken sufficiently the first impressions which it has received, in order that the difference of the numbers of oscillations operated in the two plates may be less than eight.

We find, for example, that the polarizing power of Iceland spar is expressed by 18·6, if we take that of sulphated lime as unity; or that it requires a thickness of sulphate of lime of 18·6 to destroy the modifications impressed on the rays of light by one in thickness of Iceland spar. This relation will also be that of Iceland spar, since rock crystal acts precisely like sulphated lime. This report would only be 17·7, according to other experiments of M. Malus. The difference is insensible, and M. Biot does not answer for it: all the other substances which he was enabled to subject to a similar proof, gave him the same equality with the ratio of the polarizing forces: and this ended by demonstrating to him, if demonstration was still necessary, that the theory of the oscillations of light attenuates these phænomena in their infancy, and brings them back to the consideration of the true forces by which they are produced.

In the work which the same author has published on the polarization of light, M. Biot had been led to conclude, that the luminous molecules, by passing through crystallized bodies, undergo not only geometrical deviations in the position of their axes; but that they also acquire real physical properties, which they afterwards carry with them into space, and the permanent impressions of which are manifested in experiments by affections completely novel. The proofs of this result would depend on a very delicate discussion, and would require an immense number of experiments. The author has endeavoured in his "Memoir on the physical properties acquired by luminous molecules in passing through the crystals endowed with double refraction," read to the Institute in May 1814, to exhibit less operose methods to establish a consequence so extraordinary; and the theory itself, which he deduced from it, pointed out to him the most simple means of establishing it directly.

He begins by polarizing a white ray by means of reflection on a mirror. He transmits it afterwards perpendicularly through a natural

a natural plate of sulphated lime of a thickness which exceeds $\frac{45}{100}$ of a millimetre, and of which the axis forms an angle of 45° with the primitive plan of polarization. The two bundles, ordinary and extraordinary, which result, both issue in the same direction: besides, according to the theory formerly established, these two fasciculi come out white; and if the thickness is only a few centimetres, they act as being polarized at right angles, one in the direction of the primitive polarization, and the other in a rectangular direction.

He excludes this second fasciculus by transmission through a pile of pieces of glass arranged so as to reflect it in totality, without acting in any way on the first fasciculus, which alone remains visible through the pile.

Then, if we compare the latter with a ray polarized in the same direction by simple reflection on a glass, we see that they appear perfectly similar as to the geometrical arrangement of the particles, and in the direction of the polarization; for they act absolutely in the same way, when we prove them by a prism of Iceland spar, or by reflection on an inclined glass. In the first case they are also resolved into two white images, which disappear and reappear at the same limits; in the second they are reflected in the same way, and escape both together from reflection. Also, if we make them pass through thin laminæ of sulphated lime, rock crystal for instance, they also give images coloured with the same tints; and both of them cease to give any when these laminæ have attained certain limits of thickness. But to so close a resemblance a capital difference is added: it is, that beyond these limits, the thickness always increasing, the ray polarized by simple reflection never gives colours; whereas the fasciculus which has in the first place passed through the thickness *e* of sulphated lime begins to give them over again, when the thickness of the second lamina of this subtance enters into the limits $e \pm \frac{41}{100}$ of a millimetre. It preserves, therefore, in this respect the durable trace of the physical impressions which it had at first undergone on passing through the first crystallized plate, and these impressions are relative to the thickness *e* of this plate; whereas the ray polarized by simple reflection is modified completely, as if it had passed through a plate crystallized of an infinite thickness. The difference of the two rays is also manifested in several other phænomena, indicated by theory, and which it would have been difficult if not impossible to account for otherwise.

In his previous inquiries respecting crystals endowed with double refraction, the author has shown that we may obtain fasciculi coloured, both ordinary and extraordinary, with thick as well

well as with thin plates on one and the same luminous ray. When these plates are of the same nature, the opposition is always effected by crossing at right angles their axes of double refraction. But when they are of a different nature, in certain cases the axes must be crossed, and in others they must be rendered parallel. This last case takes place, for instance, when we combine the needles of beryl with those of quartz. When the axes of these two substances are placed in the same way relative to a polarized ray, the impressions which they communicate to it are such, that if they are successive they destroy each other, and on the contrary they are continued and added together if the axes are crossed at right angles: this is precisely the inverse of what we observe when we combine two plates taken from one and the same metal. Thus, in that kind of magnetization with which crystals act on the luminous particles which pass through them, we must distinguish two modes of impression different and opposite to each other, like the vitreous and resinous electricities, or the two boreal and austral magnetisms. We may call them the quartzous and beryllated polarizations. The following is a list of some substances which belong to one or other of these denominations:

Quartzous polarization: Rock crystal, sulphated lime, sulphated barytes, topaz.

Beryllated polarization: Rhomboidal carbonated lime, arragonite, phosphated lime, beryl, tourmaline.

When we combine together two crystals the polarization of which is of the same nature, we must cross their axes in order to obtain the differences of their actions; and, on the contrary, we must render them parallel if their polarizations are different. We see that the primitive form of a crystal has no evident relation with the kind of polarization which it exercises, any more than it has with the electrical properties of the minerals.

By studying the action of *tourmaline* upon light, M. Biot recognised in it the singular property of having the double refraction when it is thin, and the simple refraction when it is thick. In order to make these phænomena clear, he polished the inclined faces of a large piece of tourmaline, so as to form with it a prism, of which the section was parallel to the axis of the needle, which is also that of the primitive rhomboid. If we look at the flame of a taper through this prism, directing the visual ray into the thinnest part, we see two images of a lustre sensibly equal, one of which, ordinary, is polarized in the direction of the axis of the tourmaline; and the second, extraordinary, is so in a direction perpendicular to this axis. But in proportion as we carry the visual ray into the thickest part of the prism, the ordinary image is weakened, and finally disappears

 entirely:

entirely: whereas the extraordinary image continues to be transmitted, without undergoing any other diminution of density than that which arises from absorption.

This property gives birth to several other phænomena, which it is easy to foresee when we know them, and which experience also confirms. They have a considerable analogy with those which Dr. Brewster discovered in agate. On examining the latter, M. Biot was convinced that they do not take place, as in the tourmaline, but beyond certain limits of thickness; for, by thinning the agate sufficiently, all the properties are restored to it which belong to crystals endowed with double refraction.

[To be continued.]

LXIX. *Intelligence and Miscellaneous Articles.*

AN IMPROVED PIANO FORTE.—NEW LOGARITHMETIC SCALES.—QUANTITY OF RAIN ON BLACKSTONE EDGE.

Mr. Editor,—Happening to have known that Mr. *Loeschman* the musical-instrument maker has been long employed, under the direction of *Earl Stanhope*, in constructing a new instrument, and having heard that it was at length completed, I called yesterday at his house in Newman Street, and was highly gratified by examining and hearing the novel and powerful effects of the *single steel strings* of his Lordship's invention: by which every desirable degree of loudness, more indeed than many would like in a small room, combined with a mellow sweetness of tone, seems accomplished, and the effect of an improved swell thereon, is excellent. The lower of his Lordship's *Strings*, or Wires rather, seem near or quite *a tenth of an inch in diameter*, and more resemble musical bars than strings, in their tone and effect. The chief difficulties have, as I understand, lain in effecting a construction of the Instrument, to bear without alteration of form, so vast a strain as these strings collectively require, and to preserve at the same time its resonant effect; and in the construction of the hammers, for duly impelling these massive strings, without too audible a blow, and rendering the *touch* too heavy.

With respect to its mode of *tuning*, it will I trust, to all competent judges of the subject, be a recommendation, and creditable also to his Lordship as a philosopher, to mention, that "Bi-equal Thirds," or "Tri-equal Quints," &c. have not been attempted to be introduced on this new Instrument; but Mr. Loeschman has been left to apply his great practical tuning skill, in doing the best that seems yet to have been effected on stringed Instruments

Instruments with only 12 sounds in the octave, and intended for general playing, viz. making the major Thirds a little sharper than perfect, perhaps 1 or 2 Σ's, and the Fifths, of course, $2\frac{1}{2}$ or $2\frac{1}{4}$ Σs* flatter than perfect, respectively.

I beg to be permitted to mention here, that since I began in 1807, to make new and more accurate reductions of Musical Intervals *from one notation to others*, than had previously been done, I have constantly used "Logometric Logarithms," that is, I constructed a table of the logarithms of the logarithms of musical ratios, and have long ago communicated the mode of making and using these to my friends *C. J. Smyth* and others. On the 17th of November last, a paper was read to the Royal Society (and which is just now published in the Phil. Trans. of 1815, part i.) by Dr. *Peter M. Roget*, wherein this important and perhaps hitherto unpublished application of logarithms is described, and they are applied on straight, spiral and diagonal Scales, to the most easy performing of numbers of the most laborious arithmetic operations by any other known means, in the involution and extraction of Roots, compound Interest, experimental Equations, Musical calculations, &c. &c.

This curious and important paper, you will, I hope, at the proper time, transfer to the pages of your useful Miscellany, and thereby oblige yours, &c.

May 13, 1815. JOHN FAREY, Sen.

P.S.—I should feel very highly obliged to your Correspondent Mr. Thomas Hanson of Manchester, who in your last number, page 317, and in vol. xliii. page 237, has given an account of *Rain* that fell on *Blackstone Edge* in the years 1814 and 1813, respectively, to either explain the mistake committed in the first of these accounts; or otherwise, throw what light he may be able, on such a most extraordinary difference, as 29·070, and 86·085 inches, in the corresponding 9 months of these two following years!!: his remarks and promise in p. 239 of the former account, embolden me to confidently expect his speedy answer hereto.

ANCIENT ASTRONOMY.

Baron de Zach, in his publication on the Attraction of Mountains, a valuable astronomical work lately published at Avignon, in two volumes 8vo, after speaking of a Trigonometrical Survey of the neighbourhood of Marseilles, and its utility in forming a topographical Map of the surrounding territory, proceeds thus:

"This affair led us to examine several interesting points in

* See Plate V, in vol. xxviii. and Corol. xviii. in p. 375 of vol. xxxvi. of Phil. Mag.

this city; among others, those where Pytheas and Gassendi made their famous observations of the solstice. These inquiries led us to a careful discussion of their celebrated observations, and to the discovery of another made by Pytheas at Marseilles, 350 years before our æra, and which have hitherto been unknown. We were only acquainted with one of his for determining the solstice, mentioned by Strabo, to whom we are indebted for its preservation. That which we have found, is the observation of an equinox; which, according as it is related by Hipparchus, ought to have been made at Byzantium (the present Constantinople), but we have proved by calculation that this observation could only have been made at Marseilles; and we refer it to Pytheas, to whom it undoubtedly belongs. These two observations, when properly computed, according to all the elements of modern astronomy, have given us the true obliquity of the ecliptic at this remote period, and its diminution after a lapse of two thousand years. The results agree with the theory in an extraordinary manner." T. S. E.

On the isochronous Rate of Going of two Clocks under certain Circumstances. By J. A. De Luc, *Esq. F.R.S.*

To Mr. Tilloch.

Sir,—I have found in your number for last March, article xxxi. a paper under the title of "On the Rate of Going of two Clocks, with Remarks on Harris's Pendulum Clock, erected in 1814, by Mr. Thomas Reid."

My present remarks shall be confined to the following passages. "The circumstance of two clocks keeping so closely together, and for a period of considerable length of time, appearing extraordinary and unexpected to me, is the reason why I have transmitted the case to you; and should you deem it worthy an insertion in your valuable journal, it is at your service.

"Both the clocks were going in an imperfect state, and were intended for further improvement; but the assuming the appearance of keeping *so near together*, made me delay the taking them down to make any alteration, till I saw how long they would continue to do so, which they did for such a length of time, and might have done so for *how long*, I do not pretend to say, had it not at last become necessary for me to put an end to it."

I have seen a similar coincidence, the cause of which was at last discovered. There were two clocks in a clock-maker's shop, one of which was his time-keeper, and an excellent instrument; the other, which was intended to be regulated by the former, did not agree when it stood at a distance: he then placed it close to the

the regulator, in order to observe them easier together; and when they were thus situated, they agreed perfectly. The clock-maker, with whom I was acquainted, communicated to me this singular circumstance, and invited me to come to his shop to witness it.

We first remarked that the oscillations of the pendula were in opposite directions; they both receded from each other, and both returned. Our first experiment was to give to their pendula the same motion: then one relented, was a moment without motion, and when it began to move, it was again in the contrary way to the other, and continued so; their contrary motions being *isochronous*. We were long without making any surmise on the cause of this singular agreement, when at last we took notice that both clocks stood on a floor, the boards of which were elastic; when therefore the pendulum, which was very heavy, was moving on one side, its weight occasioned a flexion of the board one way, which was restored when it moved on the other side, and this effect surmounted the power of the escapement. Having suspected that cause, we placed on the boards some heavy weights to render them immoveable, and the effect ceased on the clocks, each going at its proper rate.

I presume that this might have been the case with Mr. Reid's two clocks.

I am, sir,
Your obedient servant,
J. A. De Luc.

STRONTIAN IN ARRAGONITE.

M. Laugier the French mineralogist has published the following interesting notice on this subject in the *Mémoires du Muséum d'Hist. Nat.* 1r *An. or* 11e *des Annales.*

"Since Professor Haüy observed that carbonate of lime properly so called, and arragonite crystallize very differently; it has been suspected that these substances differ also in their composition, and many chemists have engaged in their comparative analysis. The greatest number, however, concluded from their experiments, that these two substances exhibit no difference in chemical analysis, and that they contain exactly the same quantities of lime, carbonic acid, and water. Of all the chemists who have operated on these minerals, Professor Stromayer has been the most successful. In May 1813 he addressed a letter to M. Haüy, describing the process by means of which he discovered the existence of strontian in arragonite, and separated it from the carbonate of lime which constitutes about 97 per cent.

cent. of arragonite. He has subsequently published a voluminous Latin memoir, detailing all the experiments necessary to confirm this discovery.

M. Stromayer's opinion being directly contradictory to those of a great number of respectable French and English chemists, M. Haüy requested me to examine the arragonite of Auvergne. I readily complied, being curious to examine a fact denied by one and affirmed by another. After a few attempts, I informed M. Haüy that I had indeed found a saline substance which could neither be nitrate of lime, as it was insoluble in alcohol, and did not become moist in the air, nor lime resulting from the decomposition of a portion of nitrate, if I might judge from its great solubility in water, which besides did not become the least turbid in contact with air. Having this substance only in the state of powder, in order to satisfy myself of its nature, I waited till by spontaneous crystallization it should present regular crystals. The first specimen of the crystals which I obtained, evinced the properties of nitrate of strontian; they are transparent, solid, unchangeable in the air, of a sharp acrid taste, of a very regular octohedral form, and give a purple colour to the flame of a taper. If these crystals were not nitrate of strontian, they might be nitrate of barytes; but if barytes was the base, the uncertainty respecting the existence of strontian in arragonite would soon vanish. On the other hand, this uncertainty could never have existed, if Professor Stromayer's process had been strictly followed, since it has been known to the French chemists, who probably overlooked the strontian because they did not use the same means of detecting it.

"The process of Stromayer is susceptible of abridgement; this chemist left exposed to the air the nitrate of lime evaporated to the consistence of honey, until it became liquefied, and the crystals of nitrate of strontian were deposited; but we may immediately treat the evaporated mass with alcohol, which dissolves only the nitrate of lime without sensibly affecting the nitrate of strontian. We may wash the insoluble crystalline powder in alcohol, then dissolve it in a small quantity of warm water, and leave it to crystallize.

"This discovery of Stromayer is an additional testimony in favour of the superior accuracy and utility of crystallography to the sciences of mineralogy and geology, and also to chemistry itself. Had not the father of crystallographic science so clearly and firmly maintained the diversity of primitive structure in arragonite and carbonates of lime, chemists would not again have thought of looking for any dissimilarity in their chemical constitution, after the number of respectable analysts who have pronounced

nounced their identity. Here also the sciences of chemistry and crystallography mutually illustrate each other; whereas the empirical methods of colours and external characters are as fluctuating and various as the number and character of observers.

VOLCANO OF ALBAY IN THE INDIAN OCEAN.

A dreadful eruption of this volcano took place on the 1st day of February 1814.

This volcanic mountain is situated in the province of Camarines, on the southern part of the Island of Lucon, or Luconia, one of the Philippine isles in the Indian Ocean.

Five populous towns were entirely destroyed by the eruption; more than twelve hundred of the inhabitants perished amidst the ruins; and the twenty thousand who survived the awful catastrophe were stript of their possessions and reduced to beggary.

The following account of this awful visitation was drawn up by an eye-witness, and intended as an appeal to the charitable feelings of the inhabitants of the Manilla Islands:

More than 13 years had elapsed, during which the volcano of Albay, by some called Mayon, had preserved a continued and profound silence, without giving the least sign of its existence. It was no longer viewed with that distrust and horror, with which volcanoes usually inspire those who inhabit the vicinity. In the year 1800 its last eruptions took place, in which it emitted a great quantity of stones, sand, and ashes, (as had always been usual,) and occasioned considerable damage to the same villages that it has now completely destroyed; rendering useless a great number of fertile fields, which thenceforth were converted into arid and frightful sands. In the latter part of October of that year the last eruption happened, and caused more damage to those villages.

Since that time we had not remarked any circumstance indicative of the existence of the volcano, and therefore all the apprehension that it had formerly inspired was gradually dissipating. Consequently, its extensive and spacious brow had been converted into a highly cultivated and beautiful garden. In particular, the inhabitants of Camalig and Budiao had planted upon it many cocoa-trees, and every kind of fruit-trees, with a variety of roots and vegetables; which, while they afforded an agreeable perspective, supplied, by their excellent productions, many industrious families with food.

In this state was the volcano on the first day of February last. No person reflected in the slightest degree upon the damages and losses that so bad a neighbour had been in the habit of occasioning. We had become persuaded, in consequence of so

 long

long a silence, that it was now completely extinguished, and that all those subterraneous conduits were closed, through which it attracted to itself and kindled the combustible materials, which it had formerly so continually thrown out. Nor had we seen or remarked any signs which might indicate to us beforehand what was about to take place. In the former eruptions, there were heard, a considerable time previous, certain subterraneous sounds, that were sure presages of them. It also exhaled almost continually a thick smoke, by which it announced them. But upon the present occasion we remarked nothing of all this. It is true, that on the last day of January we perceived some slight shocks; but we scarcely noticed them, on account of their having been very frequent since the earthquake that we experienced on the 5th of October of the year 1811. On Monday night the shocks increased. At two in the morning we felt one more violent than those we had hitherto experienced. It was repeated at four, and from that hour they were almost continual until the eruption commenced.

Tuesday dawned, and I scarcely ever remarked at Camarines a more serene and pleasant morning, or a clearer sky. I observed, however, that the ridges nearest to the volcano were covered with a mist, that I supposed to be the smoke of some house thereabouts that had been on fire in the night. At eight o'clock on that fatal morning the volcano began suddenly to emit a thick column of stones, sand, and ashes, which with the greatest velocity was elevated in a moment to the highest part of the atmosphere. At this sight we were astonished, and filled with the utmost dread, and especially when we observed that in an instant the brow of the volcano was covered by it. We had never seen a similar eruption, and were immediately convinced that a river of fire was coming towards us, and was about to consume us. The first thing that was done in my village was to secure the holy sacrament from profanation, and betake ourselves to a precipitate flight. The swiftness with which that dreadful tide rolled towards us, did not give us much time either for reflection or conversation. The frightful noise that the volcano made caused great terror, even in the stoutest hearts. We all ran terrified, and filled with the greatest dismay and consternation, endeavouring to reach the highest and most distant places, in order to preserve ourselves from so imminent a danger. The horizon began to darken, and our anxieties redoubled. The noise of the volcano continually increases; the darkness augments; and we continue our flight for the preservation of our lives, removing further and further from an object so terrific. But notwithstanding the swiftness with which we run, we are overtaken in our disastrous flight by a heavy shower of huge stones,

stones, by the violence of which many unfortunate persons are in a moment deprived of life. This unforeseen and cruel circumstance obliges us to make a pause in our career, and to shelter ourselves under the houses; but flames and burnt stones fall from above, which in a short time reduce them to ashes.

Who is capable of making an exact relation of scenes so sad and melancholy, and of presenting them to the public in the same manner that they occurred? Which of us thought to escape with life, upon beholding such manifest signals of Divine justice? As for myself, I remembered in those dreadful moments the disastrous fate of the cities of Pentapolis, and I was then persuaded that the unfortunate villages of Camarines were about to suffer the same unhappy catastrophe. Terrible reflections, it is true, but founded upon the immorality of manners which had long been remarked in those villages!

In this dreadful situation, we called upon God, in such manner as we could, from the bottom of our afflicted and almost broken hearts, beseeching him for pardon and mercy. It became completely dark, and we remained enveloped and immersed in the most thick and palpable darkness, comparable only to that which in the time of Moses was witnessed in Egypt. From this moment reflection is at an end, advice is no longer given, and no person recognises another. The father abandons his children, the husband his wife, she remembers not her beloved spouse, and the children forget their parents. No one thinks that he can assist his fellows, because all believe that they are about to die.

But as man, even in the most critical and destitute situations, endeavours by all possible methods to preserve life, each one of us, for this interesting object, makes use of all the means and expedients that can be resorted to, in the terrible condition to which we are reduced. Of what various and different methods did not we, who have escaped with life, avail ourselves, that we might not perish at that time? In the houses we now found no shelter. It was necessary to abandon them with all haste, in order not to perish with them. To go out uncovered, was to expose one's self to a danger not less imminent; because the stones that fell were of an enormous size, and fell as thick as rain itself. It is necessary, that we may not die in the one or the other manner, to cover ourselves and defend ourselves as well as we can. We do so.—Some cover themselves with hides, others with tables and chairs, others with boards and tea-trays. Many take refuge in the trunks of trees, others among the canes and hedges, and some hide themselves in a cave, which the brow of a mountain offered them. Those only of us survive, who had the good fortune to protect ourselves by one or other of those methods;

methods; but those who were in the open air, with nothing at hand with which they could cover themselves, almost all perished or were wounded.

The horrid and frightful noise of the volcano increases to its utmost; the shower of stones and thick sand augments; the burning stones and meteors continue to fall, and in a very short time reduce to ashes the most beautiful villages of the province of Camarines. Would you have signs more analogous to those that are to take place at the last judgement? The animals of the mountain descend precipitately to the villages, to seek in them a secure asylum. The domestic animals run terrified with the greatest disorder and affright, uttering cries that indicate their approaching end. Nothing interested us in those dreadful moments but the preservation of our own lives. But, alas! Divine justice has already marked and pointed out, with the finger of Omnipotence, a great number of victims, who are to perish in this day of wrath and fury, in every respect very similar to what we read in the holy Scriptures concerning the day of the last judgement.

At about ten in the forenoon it ceased to rain heavy stones, and each one endeavoured to remain in the situation he then was, waiting until the rain of thick sand which succeeded it should also cease, or until some new and unforeseen calamity should terminate the existence of us all.

We thus continued until half past one in the afternoon, at which hour the noise of the volcano began to diminish, and the horizon to clear a little, at sight of which there was revived in us the hope of life, which until then had been almost wholly extinguished. At about two in the afternoon it became entirely clear, and we began to perceive distinctly the lamentable and dreadful ravages that the darkness had hitherto concealed from us. We saw with terror the ground covered with dead bodies, part of whom had been killed by the stones, and the others consumed by the fire. Two hundred of those perished in the church of Budiao; thirty-five in a single house in that village. The joy that all felt at having preserved life through such imminent dangers, was in many instantly converted into the extremity of sorrow at finding themselves deprived of their relations, friends, and acquaintances. There, a father finds his children dead; here, a husband his wife, and a wife her husband; particularly in the village of Budiao, where there are very few who have not lost some of their nearest connexions. In another place, at every step one meets innumerable other unhappy wretches extended upon the ground, who, though not deprived of life, are wounded or bruised in a thousand ways. Some with their legs broken, some without arms, some with their sculls fractured, and others

others with their whole bodies full of wounds. Such were the mournful objects that presented themselves to us during the remainder of that afternoon, many of whom died immediately, and others on the following days; the rest remaining abandoned to the most melancholy fate, without physicians, without medicines, and in want even of necessary food.

A horrible and mournful day it was, the remembrance of which will ever be indelibly engraven upon our hearts. Not one of us then thought to escape with life. Death presented himself to us in various and frightful shapes, threatening to deprive us of life by different and horrible methods. But the powerful hand of our beneficent and sovereign God restrains him. At his commanding voice, pale Death is appalled. He trembles, groans, and leaves us. He flees, terror-stricken, to the caverns of the earth, and there begins to mourn and lament the spoils which he was about to make, and of which he has been deprived. He thought on that day to have imbrued more than usual his scythe with blood; but he was obliged to humble himself before Him who governs the empires, and at whose voice the infernal regions shake with fear.

The sad result of the misfortunes of that day has been the total ruin of five villages in the province of Camarines, and the principal part of Albay; the death of more than twelve hundred unfortunate persons, and many others severely wounded; the loss of every thing that the survivors possessed in the world, being left without houses, without clothing, without animals, without the prospect of a harvest, and without a morsel fit to eat; the mournful and unhappy fate of many, who have been left orphans, abandoned to Divine Providence; others widows, with the loss of four, five, and even more children; the total destruction of their churches and parochial houses, with every thing that they contained: in consequence of which the sacraments could not be administered to such as died of their wounds the succeeding days, and who were buried without any pomp or ceremony; and the many infants who have since been born, have from necessity been baptized with common water, because the circumstances in which we were placed did not permit it to be otherwise.

The present appearance of the volcano is most melancholy and terrific. Its side, which was formerly so cultivated, and which afforded a prospect the most picturesque, is now nothing but an arid and barren sand. The stones, sand, and ashes, which cover it, are so astonishing in quantity, that in some places they exceed the thickness of ten and twelve yards; and in the very spot where lately stood the village of Budiao, there are places in which the cocoa-trees are almost covered. In the ruined villages,

lages, and almost through the whole extent of the eruption, the ground remains covered with sand to the depth of half a yard, and scarcely a single tree is left alive. The crater of the volcano has lowered, as I judge, more than 20 fathoms; and on the south side discovers a spacious and horrid mouth, which it is frightful to look at. Three new ones are opened at a considerable distance from the principal crater, through which also smoke and ashes were incessantly emitted. In short, the most beautiful villages of Camarines and the principal part of that province are converted into a barren sand.

DISCOVERY OF A NEW CHEMICAL SUBSTANCE.

Since our last, we have received a most valuable communication on various scientific subjects from M. Van Mons of Brussels. We have only room for insertion this month of the following account of a new chemical substance. "My friend Brugnatelli writes to me: 'If we throw some indigo on burning coals, or on a heated plate of iron, there arises into the air a superb violet-coloured vapour, which I thought at first was iodine, but which an ulterior examination convinced me was a peculiar body. This vapour is condensed upon cooling into very brilliant violet-coloured crystals, which are square prisms. On bringing this vapour at the moment of its formation into contact with mercury, a soft and even concrete amalgam is the result: the metal must be heated, and the vapour ascending.' M. Brugnatelli considers this violet crystalline matter, the vapour of which is so superb, as the colouring principle, which, with fecula, forms indigo; and he calls it *indigo-gene:* he is led to believe that it is a kind of vegetable metal. When indigo has lost this principle, it no longer assumes after friction that dazzling lustre which distinguishes it. M. Brugnatelli remarks very judiciously, that if the system is persisted in of naming bodies according to their colour, this substance must still be called *iode.*"

DEATH OF MR WILLIAM NICHOLSON.

With sincere regret we announce the death of Mr. William Nicholson, many years conductor of the Philosophical Journal. He died after a lingering and painful illness at his house in Charlotte Street, Bloomsbury, on the 21st of May. The loss of this gentleman will be felt not only by his family and friends, but by the scientific world at large. He was the author of many standard works in various branches of science and experimental philosophy; and from his known talents, and profound acquaintance with every thing connected with these subjects, he was usually consulted as to the practicability and general detail of all new scientific or philosophical works, with infinite advantage to their

their inventors or projectors. His habits were studious, his manners gentle; and as his judgement was uniformly calm and dispassionate, the soundness of his opinions, in the numerous matters which were daily brought before him as a scientific umpire, was never questioned.

SUTHERLAND COAL-PIT.

The article in this department of our last number, respecting "Sutherland coal-pit," appearing to contain some mistakes, a correspondent who has visited the spot, has promised us some historical and descriptive details respecting the coal-work in question, to be given in our next number.

LIST OF PATENTS FOR NEW INVENTIONS.

To Samuel John Pauley, of Charing-Cross, and Durs Egg, of the Strand, London, for certain aërial conveyances and vessels to be steered by philosophical or chemical or mechanical means, and which means are also applicable to the propelling of vessels through the water, and carriages or other conveyances by land. —25th April, 1815.—6 months.

To Jacob Wilson, of Welbeck Street, London, for certain improvements in bedsteads and bed-furniture.—27th April.—6 months.

To William Bush the younger, of Saffron Walden, in the county of Essex, for his method for preventing accidents from horses falling with two-wheeled carriages, especially on steep declivities, superior to any hitherto known or in use.—29th April. —2 months.

To Peter Martineau junior, of Islington, and John Martineau junior, of Stamford Hill, for their new method or methods of refining and clarifying certain vegetable substances.—8th May. —6 months.

To Charles Pitt, of the Strand, London, for his method or methods for the security and safe conveyance of small parcels and remittances of property of every description, and also for the security in the formation or appendage of shoes.—11th May.—6 months.

To Samuel Pratt, of No. 119, Holborn Hill, London, for his wardrobe trunk for travellers.—11th May.—2 months.

To John James Alexander Maccarthy, of Arlington Street, for his new pavement, or method of paving, pitching or covering streets, roads, and ways.—11th May.—6 months.

To Archibald Kenrick, of West Bromwich, for certain improvements in the mills used for grinding coffee, malt, and other articles.—23d May.—6 months.

Meteorological

Meteorological Observations made at Clapton, in Hackney, from April 21 to May 15, 1815.

April 21.—The clouded sky thickened, the wind eastward, and rain began by falling in drops, and increased through the day; at night it fell copiously. The Thermometer at 9 P.M. 40°. The Barometer 28·90: it fell afterwards to 28·85°. The temperature continued about 40° all night, with wind and rain.

April 22.—Cool and rainy.

April 23.—The wind remained north-east, with smart April showers of rain and hail.

April 24.—Fair, and showers at intervals. Thermometer at 11 P.M. 39.

April 25.—Fine morning; streaks of *cirrus, cumuli* lower down in the atmosphere. It clouded in the evening, and rain came on again at night. Thermometer at 11 o'clock at night 41°. Barometer 29·70. Northerly wind.

April 26.—Fair day: the clouds were copious in the morning; but the day became clear and pleasant, with *cumuli* and light bands of *cirrus* above them. In the evening cirrocumulative *cirrostratus* in little flimsy *flocculi*. Thermometer in day above 60°. At 11 P.M. 37°. Barometer 30·21. Bright starlight.

April 27.—Northerly wind, and overcast with uniform thick cloud, and a hazy atmosphere below. Starlight between clouds by night. At midnight, Thermometer 46°. Barometer 30·13.

April 28.—Clouds, and warmer; drops of rain; fine evening.

April 29.—Much *cumulostratus* with *nimbi* occasionally from N. in the morning; fair evening, with mixed clouds and occasional light showers.

April 30.—Long gentle showers and fair intervals A.M.

May 1.—The weather became warm. This morning light breezes from south-east with *cirrus* scattered above, and ephemeral *cumuli* below, marked a change in the constitution of the air. Towards evening the denser sheets of *cirrostratus* appeared, but the night was fair and warm, with a gentle breeze. Thermometer 11 P.M. 93. Barometer falling, 29·81.

May 2.—Warm fair day, with rocklike *cumulostratus*, &c. Towards evening *nimbus* and a slight imbrification and lightning. Starlight night. Thermometer at midnight 49°. Barometer 29·88. Wind calm.

May 3.—Calm warm day; much cloudiness in the morning. When it cleared, there were large rocklike *cumuli* forming into *cumulostrati* by coalition with upper masses of flattish clouds; nimbiform haze in other places. Fine clear night. Thermometer 11 P.M. 44°. Barometer 29·84. N.

May

May 4.—Clouded over from N. early. Nimbification subsiding into *cirrostratus* in the evening; clear at 11 P.M. with Thermometer 48°. Barometer 29·81 *.

May 5.—Clouded early; hard thunder-shower.

May 6.—At four in the morning white *stratus*, but clear overhead; the cuckoo and thrush, &c. singing, and Thermometer 47°. Cloudy, and fair P.M.

May 7.—(At Oxford).—Fair day, *cumuli*, &c., and warm.

May 8.—Fine warm weather, with various modifications.

May 9.—Clouds and fair, with slight showers.

May 10.—Fair, *cumuli*, &c. and a breeze, warm.

May 11.—(At Clapton.)—Fair. Thermometer 70° midday. *Cirrocumulus* and *cirrus* in tufts, &c. The *cirrus* was particularly abundant in the afternoon. SW. At night a hard thunder-shower †. — The *Papaver orientale*, or Monk's-hood Poppy, flowered to-day.

May 12.—Warm day, with a great deal of *cirrus*, *cumulus*, &c. There was a dry feeling in the air, and a stiff breeze which carried along a great deal of dust. Fine evening. Wind SW.

May 13.—Early there was a shower: intervals of fair weather and showers throughout the day.

May 14.—Four o'clock P.M. one of the strong and partial squalls of wind from W., which accompany these showers, blew down a large elm-tree in a lane leading from Hoe-street to Whipscross in Walthamstow ‡.

May 15.—Wind from W.; much cloud. *Cumulus* in abundance was observable below a milklike whiteness caused by cirrostrativeness above. Gentle showers in the evening. Thermometer 65°, and 50° at 11 P.M. Barometer 29·95.

Five Houses, Clapton,
May 15, 1815.

THOMAS FORSTER.

* The Thermometer to-day and yesterday at midday was about 65°.

† During the latter part of the day, that appearance was discoverable in the clouds which gives them the appearance of being charged with the electric fluid. The term *Fulgoric*, suggested by one of your Correspondents, seems a very proper name for the fluid.

‡ People, in cleaning ditches which lie parallel and near to hedgerows of trees, are very apt to cut some of their roots; a circumstance which seems to cause them to give way to high wind, and to determine the direction in which they fall. The elm in question fell at right angles to the wind.

METEORO-

METEOROLOGICAL TABLE,

BY MR. CARY, OF THE STRAND,

For May 1815.

Days of Month.		Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
April	27	42	52	45	30·19	30	Cloudy
	28	45	62	46	29·90	42	Fair
	29	49	49	44	·70	31	Cloudy
	30	44	49	42	·52	26	Cloudy
May	1	45	58	45	·68	45	Fair
	2	55	68	54	·79	47	Fair, with Thunder
	3	50	68	50	·77	53	Fair [in the eveng
	4	48	60	55	·76	46	Cloudy
	5	45	63	53	·72	44	Showers, with
	6	46	66	45	·73	61	Fair [Thunder
	7	45	61	47	·72	56	Fair
	8	47	67	54	·78	58	Fair
	9	56	68	55	·87	64	Fair
	10	57	67	56	·78	63	Fair
	11	58	72	57	·72	61	Fair, with Thunder
	12	57	65	52	.65	51	Fair [in the eveng
	13	56	60	53	·72	46	Showery
	14	54	60	52	·80	66	Showery
	15	55	66	51	·84	60	Showery
	16	52	66	57	30·07	58	Fair
	17	56	70	58	·30	66	Fair
	18	57	72	62	·28	60	Fair
	19	59	70	58	29·94	56	Fair
	20	61	72	51	·75	60	Fair
	21	52	56	47	·62	60	Showery
	22	49	55	47	·89	64	Fair
	23	50	61	51	·81	57	Cloudy
	24	53	62	55	·92	56	Showery
	25	57	69	58	30·10	61	Cloudy
	26	58	73	59	·22	74	Fair

N.B. The Barometer's height is taken at one o'clock.

LXX. *Historical Memoranda respecting Experiments intended to ascertain the calorific Powers of the different prismatic Rays.* By T. S. Evans, *LL.D. F.L.S.*

To Mr. Tilloch.

Sir,—Discoveries in science have been generally made by imperceptible steps; and improvements in the arts have succeeded each other by such small gradations, that in many cases the shade of difference has scarcely been apparent. Inventions have often been brought before the public with so little alteration from others which they possessed before, that the claim of novelty could scarcely be allowed them; and advances in the state of our intellectual knowledge have sometimes been made by such simple and evident means, that it has been rather a subject of astonishment they were not known long before. Instances are not uncommon in the history of science, of two persons at the same time making the same discovery or improvement, at a distance from each other, and without ever having had the least communication: but many more are to be met with, where one person has succeeded another in the same invention, and both have been equally entitled to the palm of merit.

These reflections have arisen from considering our present knowledge of the properties of light, particularly that of the different degrees of heat communicated to the thermometer by the coloured rays arising from their decomposition by means of the prism; but they would probably have arisen from considering the history and progress of any other department of science or art, as they have all proceeded nearly in a similar way.

That light could be reflected and refracted, was well known to the ancients, as appears from the few remains that are left of their writings. Contrary to the opinion that has lately been generally entertained, the celebrated work of Ptolemy upon Optics is still in existence. A copy of it is yet in the Imperial library at Paris; and some extracts from it have been lately given by the Chevalier Delambre, in the *Connoissance des Temps* for the year 1814, by which, and the treatise of Euclid on Optics, it appears that these two properties were well understood in their times by the Greeks and Egyptians.

Long before it was certainly known that light proceeded by a progressive motion, Dr. Hook had asserted that this motion could not by any means be instantaneous; yet it may fairly be presumed that, up to the time of this discovery, mankind had no idea of any thing in nature moved with such an incredible velocity; and accordingly we find Galileo and the members of the *Academia del Cimento* endeavouring to discover what dif-

ference existed between the time of the sun's appearing to rise to a spectator situated in any given place, and to another standing two miles further off to the westward. Our present acquaintance with the astonishing rate at which it moves makes us almost smile at the attempt to discover it by such inefficient means; but without experiments, nothing can be known with certainty.

We are indebted for our knowledge of this important fact to the invention of telescopes. Mr. Olaus Römer found it from observations of the eclipses of Jupiter's satellites made at different distances of that planet from the earth; and we are now informed that light travels from the sun to us, a distance of 95 millions of miles, in about 8′ 13″ of time.

Mr. Boyle, who was indefatigable in his attention to every subject of importance connected with chemistry and natural philosophy, has informed us of a great number of curious experiments which he made on light and colours; and even at present, although our acquaintance with the subject has proceeded so much further, his work is extremely interesting.

It is, however, to the great Sir Isaac Newton that we are indebted for the most valuable knowledge which we possess of the different properties of light and colours. By making a pencil of rays pass near the edge of any sharp body, as that of a knife or broken glass, he proved that they were bent or inflected; and by making the rays pass through glass prisms, and receiving the spectrum at a distance, he found light to be a heterogeneous mixture of differently refrangible rays; and that there are as many simple or homogeneal colours, as there are degrees of refrangibility; for to every different degree of refrangibility belongs a different degree of colour.

The ideas of philosophers respecting the nature of colours before the time of Newton are extremely hypothetical, and unsupported by the least attempt at experimental proof. It may therefore easily be conceived what pleasure was given to the lovers of truth, by so extensive a pursuit of the subject as was published in his treatise of Optics. After he had thus developed the true theory of light and colours, he then proceeded to explain all their properties and phænomena, in so satisfactory a manner that his investigation has justly been considered as the best model for guiding all future inquiries into the laws of Nature.

With regard to the absorption of warmth from coloured rays,—the object of our present consideration,—Mr. Boyle caused a large block of black marble to be ground into the form of a spherical concave speculum; and found that the sun's rays reflected from it were far from being too powerful for his eyes, as would have been the case had it been of any other colour; and although its

its size was considerable, yet he could not set a piece of wood on fire with it; whereas a far less speculum, of the same form, made out of a more reflecting substance, would presently have made it flame. He took also a large broad tile, and having made one half of its surface white, and the other black, he exposed it to the summer sun for some time, when he found that while the whited part remained cool, the part that had been blacked had grown very hot. For his further satisfaction he left part of the tile of its native red; and, after exposing it to the sun, observed that this part grew hotter than the white, but was not so hot as the black part.

Mr. Melville supposed that, since there is no reflection of light but at the surface of a medium, the greatest quantity of rays, though crowded into the smallest space, would not of themselves produce any heat. If this be true, it follows that the portion of air which lies in the focus of the most powerful speculum is not at all affected by the passage of light through it, but continues of the same temperature with the surrounding air: whereas, if an opaque body denser than air be put into this place, it would be intensely heated in an instant. Hence he observes, the atmosphere is not much warmed by the passage of the sun's light through it, but rather by its contact with the surface of the earth. This he thought furnished a simple and plausible reason why it is coldest, in all climates, on the tops of high mountains; they being furthest removed from the general surface of the earth. On this principle we may also account for the difficulty which the Abbé Nollet met with in endeavouring to fire spirit of wine, oil of turpentine, olive oil, and ether; as these fluids transmit the rays, instead of reflecting or absorbing them in a sufficient degree to produce the heat necessary for their combustion.

It was remarked by Scheele, that the thermometer when filled with alcohol of a deep red colour, rose more rapidly when exposed to the sun's rays than another filled with the same kind of spirit uncoloured; but that the fluid rose equally in both when dipped together in the same vessel of warm water.

Dr. Franklin found that the hand when applied alternately to the black part of any one's dress, and then to a white part, whilst walking in the sun, would feel a great difference in their warmth. He observed also that black paper exposed to the concentrated rays of the sun from a lens, would take fire much sooner than white paper; and that a fluid of any kind becomes warm in less time, when placed before the fire in a black vessel, than in a white one, or in a bright silver tankard. But the most curious experiment he made was, by exposing pieces of broad-cloth of various colours to the sun, by laying them upon snow. In a few hours the black, being warmed most by the sun, was sunk so

low as to be below the sun's rays; the dark blue almost as low; the lighter blue not quite so low as the dark; the other colours less, as they were lighter; and the white remained on the surface of the snow, not having sunk at all*.

It is easy to conceive that the next step in their progress to ascertain the various properties of light and heat, would be to examine whether rays of different refrangibility give a different degree of heat to those bodies with which they come in contact; and the best and most rational mode of making this evident to the senses, would be by causing the coloured rays themselves to fall immediately on the bulbs of thermometers placed to intercept the rays. This was first attempted, I believe, by the Abbé Rochon, in the years 1775 and 1776.

The experiments of this gentleman appear to have been passed over with little or no notice, although they form an important link in the progress of this department of science. The air thermometers which he contrived for showing the effect of the heat on them were extremely ingenious; yet, it must be owned his results differ materially from those of later philosophers: still, however, I think it will be admitted that he is certainly entitled to the honour of having proved, beyond a doubt, that a different degree of heat is given to the thermometer by the differently coloured rays of light after they have been decomposed by means of the prism. As he appears to have conducted these experiments with the utmost care, and certainly could have had no reason to falsify his observations, it is not impossible but that in endeavouring to discover the cause, and to reconcile the apparent disagreement between his results and those of others who have followed him, some useful fact may arise, which at present remains unknown.

The reason why these experiments of his have not been more noticed, may perhaps arise from the circumstance of the work not being very commonly to be met with in this country; and as the memoir is but a short one, and may probably be deemed interesting by some of your readers, I have translated it, and take the liberty of sending it you for your valuable Magazine, should you deem it worthy of being inserted therein†.

It forms an important link between the brief sketch I have drawn of optical discoveries, and the highly important and later ones of Dr. Herschel, Sir Henry Englefield, Dr. Wollaston, Sir

* The greater part of your readers must be acquainted with the very ingenious experiments of Professor Leslie, proving the converse of these results, that dark-coloured bodies give out their heat much quicker than light-coloured or polished bodies—a fact of great practical utility, but not necessary to be further dwelt upon in this place.

† See the next article.

Humphry

Humphry Davy, &c. &c. and more lately those of Malus, Biot, Leslie, Brewster, &c. &c.

The work in which those experiments are printed was published in octavo, at Paris, in 1783; and is entitled *Recueil de Mémoires sur la Mécanique et la Physique.* It contains several other memoirs on heat, light, optical instruments, mechanics, and engraving: the principal of them are as follows:

On a new micrometer made of rock crystal, and additions to ditto.

On the means of employing the double refraction of rock crystal for the precise measurement of small angles; determination of the diameters of the planets by this method; with a brief description of an instrument intended to measure with accuracy the moon's angular distance from the stars, when this distance does not exceed twenty degrees. Continuation of the preceding researches.

A certificate and statement of the prior claim which the Abbé Rochon had to the invention of the prismatic micrometer in preference to the Abbé Boscovich, signed by Borda, Bezout, Vandermonde, and Cassini.

A table, serving to determine angles which result from two prisms laid one on the other.

An essay on the measurement of angles, by means of prisms of rock crystal and of glass: determination of the diameters of the planets by these methods; and different inquiries, which principally concern the analysis of colours and achromatic telescopes.

Inquiries respecting the nature of the light of the fixed stars.

On glasses having spherical surfaces; their aberration; lenses of spirit of wine, and other kinds of fluids, by which the effects of the internal surfaces of glasses are considerably diminished.

Account given of the examination of these glasses, by Borda, Le Gentil, and Cassini the younger.

On vision; and the result of the examination of his experiments on this subject, and other questions relating to the functions of this organ, by Bezout, Bory, and Borda.

Of the optical diasporameter, an instrument for measuring the dispersion of colours.

On the means of rendering M. Bouguer's heliometer capable of measuring angles of a considerable size, in order to facilitate the observations of distances of stars from the moon.

Description of a method of rendering Hadley's quadrant susceptible of measuring all angles, from 0 degrees to 180 degrees.

Report of Messrs. Lemonnier, Le Roy, and Condorcet, on two micrometers invented by the Abbé Rochon.

 Memoir

Memoir on a new micrometer and megameter, presented to M. de Sartine, by the Abbé Boscovich.

A new prismatic micrometer, for measuring small angles, invented in 1776 by the Rev. Neville Maskelyne.

A letter to Mr. Maskelyne, from the Abbé Rochon, on his prior claim to this discovery.

A memoir on a new instrument for measuring the sun's solsticial altitudes with great precision, and generally all other large angles.

On the measurement of the dispersion and refraction of different substances; and a description of the instrument which was used in this determination.

A table of the refraction and dispersion of different substances.

Description of a machine for engraving; and the report made of it to the Academy of Sciences by Messrs. Condorcet and Bossu.

An essay on the degrees of warmth of coloured rays (the subject of this memoir).

Reflections on achromatic telescopes: with eleven plates.

Some of these papers, for the reason above mentioned, are probably as little known as the one I have sent you; scarcely any references to them are to be met with in English works. They all appear to be curious, and deserving the attention of those whose studies or amusements may be directed to subjects on which they treat. As it is always useful to know where information can be obtained at those times, a sketch of them is here given for that purpose.

If the whole course of experiments contained in the Abbé's memoir be collected into one point of view, they will stand as follows:

	Violet.	Orange.	Yellow Orange.	Green.	Red.
June.	The Therm. varying only from 16 to 17 degrees.				
	4·5				40
	6				40
	4·5				43
	5·5				39
	7				44
Mean of observ. made in June.	5·5				41·2

Table

Table continued.

	Violet.	Orange.	Yellow Orange.	Green.	Red.
July.	The Therm. having varied only from 18° to 20°.				
	3			31	38
	4·5			39	40
	3·75			27·5	37
	5			30	36·5
	6		49	33	40
	7		52	34·5	39·5
	6·5		48	36	45
	8		64	48	60
Mean of observ. made in July.	5·47		53·25	34·88	42·0
August.	Therm. rose from 19° to 23° during the observ.				
	7	60		43	56
	6		62	39	55
	7	38		45	64
	5	51		39	48
Mean of observ. made in August.	6·25	49·67	62	41·5	55·75
September.	With the Air Therm. shown in fig. 5, Pl. VIII. on which the exterior air had no influence; Reaumur's Thermometer placed externally varying from 19° to 16°.				
	2		18	13	17
	2	17		14	18
	2·5	22		16	21
	2·5	20		16	22
	6				40
	4				42
	5·5				39
	7·25				44
	3			31	38
	4·5			29	40
	3·75			27·5	37
	5			30	36·5
	6	49		33	40
	7		52	34·5	39
	6·5	48		36	42
	6	50		35	43·5
Mean of observ. made in Sept.	4·59	34·33	35	26·25	34·94
General mean of them all.	5·117	39·444	49·286	31·667	40·121

As

As the thermometer used in making these experiments was of quite a different kind from any of those in common use, it does not appear possible, at present, to make the least comparison of them together. ' Had the author been aware of the necessity of doing this, it might perhaps have been effected by observing what degree of warmth communicated very slowly would produce a certain alteration in each; as the standard of comparison being thus once made, would serve at any future period for reducing the terms of the one to those of the other.

The difference in the results of these experiments and those of Dr. Herschel, it must be acknowledged, is very great; and perhaps may require some further acquaintance with the laws of Nature, before they can be reconciled. According to the very interesting experiments of the latter gentleman, the greatest heat was found to be beyond the coloured spectrum, and on the red side. In these experiments the greatest degree of heat was obtained from the yellow orange rays; and next to that from the red: then from the orange; next to that from the green: and least of all from the violet. The prism which he used was different from that used by Dr. Herschel and others, as it had its refringent angle 45° instead of 60°. At first sight this does not appear to be a sufficient cause for so wide a difference in the results; and yet, if both are correct, there must be some rational mode of accounting for their discrepancy, depending upon scientific and satisfactory principles. In these experiments the Abbé caused the rays to pass through a lens; and if we refer to the figure of his apparatus, which held the air thermometer, we shall immediately perceive that the rays had then to pass through the glass of which the bent tube was made; and probably through the outer cylindrical glass receiver, that held the fluid for the purpose of keeping it of an uniform temperature. These tubes being nearly of a cylindrical shape, the rays, by not always impinging upon them in precisely the same, or in a proper manner, might be refracted in a different angle from that in which they quitted the prism, and thereby have their nature changed, and consequently the degree of warmth might on that account be found different from what it has since been determined.

Many objections may certainly be made against the mode in which he has conducted these experiments; and indeed they are very candidly stated by himself in his account of them; for he says, that although he took all possible precautions, that depended on himself, to ensure their success, yet he was still very far from being satisfied with his labour. All that can therefore be done, until the difficulty of reconciling them with others has been removed, is to state the Abbé's just claim to the discovery, that a different degree of heat is produced by the absorption of the

the differently coloured rays of light after having been decomposed by means of the prism. To this claim he certainly appears entitled, notwithstanding the great want of agreement which at present apparently exists between his results and those of others.

We have many instances on record, of things that appeared at first sight to be of a very discordant nature, proving after further investigation to be perfectly reconcileable with themselves and with truth; an instance of which may be taken from a subject stated further back, of the discovery of the progressive motion of light by Römer. The several discussions that took place respecting it from the year 1675 to 1707, by Messrs. Cassini, Maraldi, and the discoverer, show how difficult it is to bring the scattered fragments of a noble edifice to agree, and build them up again upon a firm and immoveable foundation. When all the parts are found and made out that belong to the theory of light, heat, and colours, then perhaps, and not till then, these of the Abbé Rochon may find their place among the rest in the general arrangement of the whole.

The ratio of the warmth of clear red to the most lively violet, of 8 to 1, which he deduces from his experiments, does not appear to be admissible, unless the point 0 of the scale of his thermometer showed the commencement of all accession of caloric in the fluid of which it was composed. Thus the ratio of 5·117 to 40·121 may be expressed in round numbers by that of 1 to 8; but if the point of congelation, or whatever other point may be deemed that where the first accession of heat begins, be δ degrees below the zero of the thermometer, then the ratio of $\delta + 5{\cdot}117$ to $\delta + 40{\cdot}121$, is no longer that of 1 to 8, but approaches so much the nearer to a ratio of equality as δ increases. Let α and β be the two original heights; then the excess of the latter ratio above the former will be $\frac{\delta(\beta-\alpha)}{\beta(\delta+\beta)}$ when zero is reckoned δ degrees below.

The heliostata, which he regrets the want of, is an instrument contrived by Gravesande, and very fully described by him in his Mathematical Elements of Natural Philosophy, vol. ii. page 107, edit. of 1747. The intention of it is to avoid the double inconvenience arising from the obliquity of the rays, and the sun's diurnal motion. On account of the obliquity of the rays, some experiments can only be made at certain hours; and others cannot be made at all in particular places, although exposed to the sun a considerable portion of the day. This machine consists of two principal parts, a plane metallic speculum, and a clock, by which it is kept directed always towards the sun. A metallic speculum is used to avoid the double reflection from a glass one. The plane of the clock is inclined to the horizon in an angle equal

equal to the complement of the latitude of the place; and the arm that moves the speculum acting in the direction of this plane, will evidently cause it to follow the sun in its diurnal motion, whatever may be its declination. As the experiments concerning light and colours must be made in the dark, the whole machine, when made use of for this purpose, must be shut up in a case or box; and we may have the rays thrown by means of it in any required direction during the whole of the time he is above the horizon, if the situation be open; or, if otherwise, as long as the rays are not intercepted by surrounding objects.

Another instrument of this sort is described by Mr. Benjamin Martin, in page 469 of vol. ii. of his Mathematical Institutions, which he considers as an improvement of one described by M. Klingenstiern in the Petersburg Commentaries for 1747 and 1748: it may also be found in his *Philosophia Britannica*, p. 89, vol. iii. edit. of 1771: but the expense of such a machine it is feared will prevent its being commonly applied, although it would be extremely useful for moving telescopes round upon a polar axis, in making observations of the heavenly bodies, and pursuing them, in an uniform manner, notwithstanding the earth's diurnal motion.

The late ingenious Mr. Ramsden, whose care in the construction, and accuracy in the execution of astronomical instruments, could only be equalled by his genius and originality in inventing them, had once an idea of adapting an instrument of this sort to a clock that kept time very exactly, with a view to avoid the necessity of using *time* in particular cases of astronomical admeasurements.

Christ's Hospital, June 7, 1815.

LXXI. *An Essay on the Degree of Warmth of coloured Rays. By M. l'Abbé* Rochon, *late Member of the Academy of Sciences of Paris, &c. &c.**

In this memoir we confine ourselves to examine, whether the rays of light of different refrangibility produce degrees of heat that differ in their degree of sensibility on the thermometer.

This research requires without doubt that the colours should be distinct and well separated; but this analysis of them cannot be made without diminishing prodigiously the intensity of the light; which, when thus decomposed, no longer gives any sensible sign of warmth in our common thermometers.

It is therefore necessary to resolve or to collect on the ball of a ther-

* For some remarks on this paper, see the preceding communication from Dr. Evans, page 401.

a thermometer a sufficient quantity of these rays, to produce effects that are evident; or else to use thermometers of a sensibility proportionate to the loss of intensity which is inseparable from the light thus decomposed. We can always collect into a small space a quantity of rays of a colour that will be sensibly homogeneous, either by disposing in a proper manner several prisms of different angles which shall make each species of rays to coincide on the ball of a thermometer; or by using the burning mirror of M. de Buffon, known by the name of the Mirror of Archimedes, whose burning line is very nearly the same for a considerable length.

This instrument is an assemblage of small plane mirrors, which by their respective positions will cause as large a quantity of light as may be desired to fall on a prism placed in their common focus. Now the united rays which form this focus, or rather this burning line, approach so much the nearer to the parallelism necessary for the success of the experiment in question, as the distance of the prism from the mirrors is more considerable.

We know that this method requires a large room; but the one in which the experiments I am going to relate were made, not being of a proper size, I thought that very sensible thermometers would sufficiently fulfil the object which I proposed.

Those which I have used were air-thermometers. It is well known that they owe their effect to a bubble of air introduced into the ball which contains the fluid. The slightest degree of warmth dilates this bubble, and causes the spirit of wine to mount in the capillary tube of the thermometer: but this ascent would scarcely be sensible if the top of the tube were hermetically sealed; it is therefore necessary to leave it open, and consequently the exterior air, which is susceptible of many variations, acts on the fluid in such a way that it is necessary to have recourse to the barometer to know the effect of the dilatation, which is found to correspond with the greater or less elasticity of the exterior air.

These defects, joined to the evaporation of the liquor, obliged me to reject these thermometers, notwithstanding their prodigious sensibility. Although in my later researches I have not had these inconveniences to discourage me, it being sufficient to compare the different effects of each kind of rays that took place in a very short space of time, I have nevertheless endeavoured to avoid them altogether, by soldering to the tube of the thermometer a large ball filled with air, which contains a common thermometer. This ball is dipped into a fluid whose temperature is easily kept uniform, whether we choose to have it at the freezing point or at any other temperature.

By

By this method the full degree of sensibility is given to the thermometer, without its having any communication with the external air.

For experiments of so delicate a nature we know that a perfectly serene sky is necessary, and an unvarying temperature without the least agitation of the air: this necessarily reduces the experiments on which we may depend to a very small number; and among the great variety of those I have made, I acknowledge that there is not so much as one with which I am completely satisfied.

I used a large prism of flint glass, of which the refringent angle was 45°; I permitted an equal quantity of rays to pass through in each case, and they successively traversed the same lens, which I inclined according to the sun's altitude; for I had at that time no heliostata, and the want of this instrument has rendered my results still less accurate.

All my experiments were made between the hours of eleven in the morning and two in the afternoon; my air-thermometer was too sensible to allow of my approaching it without occasioning variations, so that I observed the divisions by a small telescope at the distance of fifteen feet, and I raised or lowered the indexes by means of a string passing over some pulleys.

Although from the year 1775 I have made a great number of experiments, I do not think them worthy of being related, because I was not then sufficiently acquainted with the precautions that are necessary in such delicate inquiries: those made in 1776 are more conclusive; they give the ratio of the warmth of the red rays to the violet rays as 8 to 1; for, by warming the ball of the thermometer for two minutes with these two kinds of rays, the index would mark from four divisions and a half to seven, for the violet rays; whilst for the red rays it was necessary to place the index from 39 to 44 divisions. These observations were made in the month of June, Reaumur's thermometer only varying during the whole time they were made from 16° to 17°.

The following are the observations of this month:

Violet $4\frac{1}{2}$	Red 4*
6	40
$4\frac{1}{2}$	43
$5\frac{1}{2}$	39
7	44

It is necessary to remark, that it is the red approaching to the orange, and the violet approaching to the blue, that I have used. It is so difficult to decompose the light with sufficient perfection to obtain the same shade of colour constantly, that we must not

* Most probably this is an error of the press, and ought to be 40 It is so considered in the general result given in page 406.

be

be surprised at the variations I have experienced, although they are considerable.

In the month of July of 1776, the thermometer having only varied from 18° to 20° during the course of the experiments:

The violet was	3	Green	31	Red	38
	4½		39		40
	3¾		27½		37
	5		30		36½

Violet	6	Yellow orange	49	Green	33	Red	40
	7		52		34½		39½
	6½		48		36		45
	8		64		48		60

In the month of August Reaumur's thermometer rose from 19° to 23°, during the following observations:

Violet	7	Orange	60	Green	43	Red	56
Violet	6	yellow orange	62	green	39	red	55
Violet	7	orange	38	green	45	red	64
	5		51		39		48

In the month of September, I used a thermometer of the construction represented by fig. 5, Plate VIII. on which the exterior air had no influence; the bubble of air was always similarly exposed, for two minutes, to the action of the coloured rays, as in the preceding experiments; but the sensibility of the thermometer no longer continued to be so great.

In the month of September, Reaumur's thermometer varied from 19° to 16° in the course of the observations:

Violet	2	Yellow orange	18	Green	13	Red	17
	2	orange	17		14		18
	2¼	orange	22		16		21
	2½		20		16		22
	6						40
	4						42
	5½						39
	7¼						44
	3				31		38
	4½				29		40
	3¼				27½		37
	5				30		36½
	6	orange	49	green	33		40
	7	yellow orange	52		34½		39
	6½	orange	48		36		42
	6		50		35		43½

It follows from all these experiments, and notwithstanding the great differences which are to be found amongst them, and however attentive I was in making them; first, that the ratio of the warmth of clear red to the most lively violet, is nearly as 8 to 1, for

for the extreme rays cannot be compared. Secondly, that the warmth of the yellow orange differs but little from that which the red produces: so that it may be presumed that the warmest rays are between the clear red and the yellow: and from this point the warmth of the rays diminishes considerably more on the violet side than on that of the deep red.

I was desirous of using coloured liquids and glasses, but both these substances, when coloured, transmit heterogeneous rays in too great a quantity to be used with success in experiments of so delicate a nature: besides, How are we to estimate the reflected rays, and those that are lost in these substances? In other respects, although I have taken all possible precautions, that depended on myself, to ensure the success of these experiments, I am still very far from being satisfied with my labour.

LXXII. *Some Experiments and Observations on the Colours used in Painting by the Ancients. By Sir* HUMPHRY DAVY, *LL.D. F.R.S.*

[Concluded from p. 359.]

VI. *Of the Purple of the Ancients.*

THE πορφύρα of the Greeks, and the ostrum of the Romans, was regarded as their most beautiful colour, and was prepared from shell-fish.

Vitruvius* says that the colour differed according to the country from which the shell-fish was brought; that it afforded a colour deeper and more approaching to violet from the northern countries, and a redder colour from the southern coasts. He states, that it was prepared by beating the fish with instruments of iron, freeing the purple liquor from the shell containing it, and mixing it with a little honey: and Pliny says, that for the use of the painters argentine "creta†" was dyed with it: and both Vitruvius and Pliny say, that it was adulterated, or imitations of it made, by tingeing "creta" with madder‡, and "hysginum." The finest purple, Pliny says, had a tint like that of

* Lib. vii. cap. 13.

† Probably a clay used for polishing silver. The ancients were not acquainted with the distinction between aluminous and calcareous earths, and *creta* was a term applied to every white fine earthy powder.

‡ Madder was extensively used by the ancients in dyeing, and from this passage it is probable that they were acquainted with the art of making a lake from it similar to that used by modern painters. It was probably one of the colours used by the Egyptians in dyeing their stuffs of different colours from the same liquor, by means of mordants. If we can trust Pliny's account, they practised calico-printing in a manner similar to the moderns. Lib. xxxv. cap. 42.

a deep-

a deep-coloured rose: and in painting, he states that it was laid on to give the last lustre to the sandyx, a composition made by calcining together red ochre and sandarach, and which therefore must have been nearly the same as our crimson.

In the baths of Titus there is a broken vase of earthenware, which contains a pale rose colour; where it has been exposed to air, it has lost its tint, and is become of a cream colour, but the interior has a lustre approaching to that of carmine.

I have made many experiments on this colour. It is destroyed and becomes of a red brown by the action of concentrated acids and alkalies; but diluted acids dissolve a considerable quantity of carbonate of lime with which the body colour is mixed, and leave a substance of a bright rose colour: this substance when heated first blackens, and when urged with a strong flame becomes white; and treated with alkali, proves to be composed of siliceous, aluminous, and calcareous earths, with no sensible quantity of any metallic substance, except oxide of iron.

I endeavoured to discover if the colouring matter was combustible. It was gradually heated in a glass tube filled with oxygen; it did not inflame, but became red hot sooner than it would have done had it been merely earthy matter: on exposing the gas in the tube to lime-water, there was a precipitation of carbonate of lime. Some of it was mixed with hyperoxymuriate of potassa, and heated in a small retort; when the salt fused there was a slight scintillation, a little moisture appeared, and the gas given off received into lime-water occasioned a very evident precipitation.

It appeared from these experiments, that the colouring matter was a compound of either vegetable or animal origin. I threw some of it upon a hot iron: it emitted scarcely any smoke, and gave a smell which had some resemblance to that of prussic acid, but which was extremely faint.

When hydrate of potassa was fused in contact with it, the vapours that rose had no distinct ammoniacal smell; they gave indeed slight fumes to paper moistened with muriatic acid, but this is far from being an unequivocal proof of animal matter. I compared this colour with vegetable lake from madder, and animal lake from cochineal diluted to the same degree as nearly as could be judged, and fixed upon clays. The lake of madder, after being dissolved in strong muriatic acid, had its colour restored by alkalies, which was not the case with the ancient lake. The lake of madder likewise gave a much deeper tint to muriatic acid, and produced a tawny hue when its weak muriatic solution was acted on by muriate of iron; whereas the ancient lake did not change in colour. The ancient lake agreed with the lake of cochineal in being rendered of a deeper hue by weak alkalies, and

and of a brighter hue by weak acids; but it differed from it in being much more easily destroyed by strong acids. It agreed with both the vegetable and animal lakes in being immediately destroyed by a solution of chlorine.

The lake made from cochineal produced much denser fumes when exposed to fused potash, and afforded a distinct ammoniacal smell. The two modern lakes when burnt in oxygen did not give stronger signs of inflammation than the ancient. I ascertained the loss of weight this ancient lake suffered by combustion, and found it only $\frac{1}{30}$, and this loss must in great part have depended on the expulsion of water from the clay on which it was fixed. This circumstance induced me to renounce the idea of attempting to determine its nature from the products of its decomposition; which in the case of so small a quantity of matter diffused over so large a quantity of surface could not have afforded unequivocal results.

The durability of this lake, whether vegetable or animal, is a very curious circumstance; but the exterior part which has been exposed to air has suffered.—This durability probably depends in a great measure upon the attractive powers of so large a mass of alumina; for, whenever one proportion of a substance is combined with many proportions of another substance, it is very difficult to decompose or detach the one proportion.

From the circumstances which have been noticed respecting this colour, it is impossible to form an opinion whether it is of vegetable or animal origin. If of animal origin, it is most probably the Tyrian or marine purple: and by some comparative experiments on the purple obtained from shell-fish the question might perhaps be decided*. It is very probable that the most expensive colour would be employed for ornamenting the imperial baths; and it is not impossible that Pliny may have alluded to the palace of the Cæsars when he says "nunc et purpuris in parietes migrantibus, et India conferente fluminum suorum limum, et draconum et elephantorum saniem, nulla nobilis pictura est." Lib. xxxv. cap. 32.

I have seen no colour of the same tint as this ancient lake in any of the ancient paintings in fresco. The purplish reds in the baths of Titus are mixtures of red ochres and the blues of copper.—In the Aldobrandini picture there is a purple in the

* M. Chaptal considers the lake he found amongst the colours from Pompeii (as I have already mentioned) as of vegetable origin; and he founds his opinion upon the circumstance of its not affording by decomposition the smell peculiar to animal substances: but probably this smell, even if produced by recent purple colouring matter of animal origin, would not belong to colouring matter of 1700 years old. For it is most probably owing merely to albumen or gelatine not essential to the colouring particles, and much more rapidly decomposed.

garment

garment of the Pronuba, but of an inferior hue; and this purple appears to be a compound mineral colour of the nature of these.—It was not destroyed by solution of chlorine; and when a little of it was exposed to muriatic acid, it rendered the acid yellow, and the remainder yielded a residual blue powder.

VII. *Of the Blacks and Browns of the Ancients.*

There is one chamber in the baths of Titus of which the groundwork is black. I have found several fragments of stucco painted black both in the baths of Titus and in the vineyard above mentioned, and also in some ruins near the Porta del Popolo.—I scraped off some of these colours and submitted them to experiments: they were not acted on by acids or alkalies, they deflagrated with nitre, and had all the properties of pure carbonaceous matter.

I found no blacks, but three different shades of brown in the vase of mixed colours; one was snuff-colour, one deep red brown, and the third a dark olive brown. The two first proved to be ochres which had been probably partially calcined; the third contained oxide of manganese, as well as oxide of iron, and afforded chlorine when acted on by muriatic acid.

All the ancient authors describe the artificial Greek and Roman blacks as carbonaceous, and made either from the powder of charcoal or the decomposition of resin, (a species of lampblack,) or from the lees of wine, or from the common soot of wood fires. Pliny mentions the inks of the cuttle-fish, but says, "ex his non fit*." Some years ago I examined this substance, and found it a carbonaceous body mixed with gelatine. Pliny speaks of ivory-black as invented by Apelles; he says likewise that there is a natural fossil black, and another black prepared from an earth of the colour of sulphur. Probably both these substances are ores of iron and manganese.

That the ancients were acquainted with the ores of manganese is evident from the use made of it in colouring glass. I have examined two specimens of ancient Roman purple glass, both of which were tinged with oxide of manganese.—Pliny speaks of different brown ochres, and particularly of one from Africa, which he names *Cicerculum*, which probably contained manganese: and Theophrastus mentions a fossil † which inflamed when oil was poured upon it, a property belonging to no other fossil substance now known but the *black wad*, an ore of manganese, and which is now found in Derbyshire.

The browns in the paintings in the baths of Livia, and in the

* *i. e.* the atramentum.

† Theophrastus says it is like decomposed wood, παρόμοιος ὢν ξύλῳ σαπρῷ, 12th page of John de Laet's edition.

Aldobrandini picture, are all produced by mixtures of ochres with blacks. Those in the Aldobrandini picture yield oxide of iron to muriatic acid, but the darker shades were not touched by that acid, nor by solution of alkalies.

VIII. *Of the Whites of the Ancients.*

The white colours in the Aldobrandini picture are soluble in acids with effervescence, and have the characters of carbonate of lime.

The principal white in the vase of mixed colours appears to be a very fine chalk. There is another white with a tint of cream colour, which is a fine aluminous clay.

The whites that I have examined from the baths of Tîtus, and those from other ruins, are all of the same kind.

I have not met with ceruse amongst the ancient colours, though we know from Theophrastus, Vitruvius, and Pliny, that it was a common colour: and Vitruvius describes it as made by the action of lead upon vinegar.

Several white clays are mentioned by Pliny as employed in painting, of which the Parætonium was considered as affording the finest colour.

IX. *Of the Manner in which the Ancients applied their Colours.*

It appears from Vitruvius that the colours used in fresco painting were applied moist to the surface of a stucco* formed of powdered marble cemented by lime; he states that the wall or ceiling had three distinct coatings of stucco made of this material, of which the first contained coarse powder of marble, the second the finer powder, and the third the finest powder of all, and that after this the wall was polished before the colour was applied. The stuccos that remain in the ruins of the baths of Titus and Livia are of this kind, and so is the ground of the Aldobrandini picture; they are beautifully white, and almost as hard as marble, and the granular marble of different degrees of fineness may be distinguished in them. This circumstance indeed offers a test of the antiquity of ruins at Rome. In the houses that have been built in the middle and later ages, decomposing lava has been mixed with the calcareous cement instead of granular marble, and the stuccos of these houses are gray or brown, and very coarse in their texture.

Pliny says that purple, orpiment, ceruse, the natural azure, indigo, and the meline white, were injured by application to wet stucco, which is easily explained in the case of orpiment, carbonate of copper, ceruse, and indigo, from their chemical composition.

* Lib. vii. cap. 2, 3, & 4.

Vitruvius

Vitruvius states that in fresco painting vermilion changed if exposed to light, and he recommends the encaustic process for fixing the colour under this circumstance, namely, laying over it a coat of punic wax, and liquefying the wax so as to make a varnish for the colour.

Pliny describes this process as applied in painting ships; and we know from his authority that several pictures of the great Greek masters were painted in encaustic, and that the different colours were laid on mixed with wax. I have examined several pieces of the painted stuccos found in the different ruins, and likewise the Aldobrandini picture, with a view of ascertaining if any application had been made to fix the colour; but neither by the test of alcohol, nor by heat, nor by the action of water, could I detect the presence of any wax varnish, or animal or vegetable gluten.

The pot of colours to which I have already referred, found at Pompeii, was blackened by smoke, as if it had been recently on a fire of wood. I thought that this might be owing to some process for dissolving gluten or varnish in the preparation of the colour; but I could detect no substance of this kind mixed with the colouring matter.

Pliny states that gluten (our glue)* was used in painting with blacks: and this specific mention of its application would induce the belief that it was not employed with other colours, which adhered without difficulty to, and were imbibed by, a surface so polished and well prepared as the Roman stucco; and the lightness of carbonaceous matter alone probably rendered this application necessary.

X. *Some general Observations.*

It appears from the facts that have been stated, and the authorities quoted, that the Greek and Roman painters had almost all the same colours as those employed by the great Italian masters at the period of the revival of the arts in Italy. They had indeed the advantage over them in two colours, the Vestorian or Egyptian azure, and the Tyrian or marine purple.

The azure, of which the excellence is proved by its duration for seventeen hundred years, may be easily and cheaply made; I find that fifteen parts by weight of carbonate of soda, twenty parts of powdered opaque flints, and three parts of copper filings strongly heated together for two hours, gave a substance of exactly the same tint, and of nearly the same degree of fusibility, and which, when powdered, produced a fine deep sky blue.

* Lib. xxxv. cap. 25. "Omne atramentum sole perficitur, librarium gummi tectorium glutino admixto."

The

The azure, the red and yellow ochres, and the blacks are the colours that seem not to have changed at all, in the ancient fresco paintings. The vermilion is darker than recently made Dutch cinnabar, and the red-lead is inferior in tint to that sold in the shops. The greens in general are dull.

The principle of the composition of the Alexandrian frit is perfect; namely, that of embodying the colour in a composition resembling stone, so as to prevent the escape of elastic matter from it, or the decomposing action of the elements; this is a species of artificial lapis lazuli, the colouring matter of which is naturally inherent in a hard siliceous stone.

It is probable that other coloured frits may be made, and it is worth trying whether the beautiful purple given by oxide of gold cannot be made useful in painting in a densely tinted glass.

Where frits cannot be employed, metallic combinations which are insoluble in water, and which are saturated with oxygen or some acid matter, it is evident from the proof of a duration of seventeen centuries, are the best pigments. In the red ochres the oxide of iron is fully combined with oxygen, and in the yellow ochres it is combined with oxygen and carbonic acid; and these colours have not changed. The carbonates of copper which contain an oxide and an acid have changed very little.

Massicot and orpiment were probably the least permanent amongst the ancient mineral colours.

Of the colours, the discovery of which is owing to the improvements in modern chemistry, the patent yellow is much more durable than any ancient yellow of the same brilliancy; and chromate of lead, an insoluble compound of a metallic acid with a metallic oxide, is a much more beautiful yellow than any possessed by the ancients, and, there is every reason to believe, is quite unalterable.

Scheele's green (the arsenite of copper), and the insoluble muriatic combination of copper, will probably be found more unalterable than the ancient greens; and the sulphate of baryta offers a white superior to any possessed by the Greeks and Romans.

I have tried the effect of light and air upon some of the colours formed by the new substance iodine. Its combination with mercury offers a good red; but it is, I think, less beautiful than vermilion, and it appears to change more by the action of light.

Its compound with lead gives a beautiful yellow, little inferior to the chromate of lead; and I possess some of this colour which has been exposed to light and air without alteration for several months.

In many of the figures and ornaments in the outer chambers of

of the baths of Titus, where only outlines or spots remain, or shades of ochre, it is probable that vegetable or animal colours, such as indigo and the different dyed clays, were used*.

Pliny speaks of the celebrated Greek painters as employing only four colours. "Quatuor coloribus solis immortalia illa opera fecere: ex albis Melino, ex silaceis Attico, ex rubris Sinopide Pontica, ex nigris atramento, Apelles, Echion, Melanthius, Nicomachus, clarissimi pictores†:" but as far as Apelles and Nicomachus are concerned, this is a mistake; and it is not unlikely that Pliny was misled by an imperfect recollection of a passage in Cicero, who describes the earlier Greek school as using only four colours, but the later Greek painters as perfect masters in all the resources of colouring. "Similis in pictura ratio est: in qua Zeuxim, et Polygnotum, et Timantem, et eorum, qui non sunt usi plus quam quatuor coloribus, formas et lineamenta laudamus: at in Aetione, Nicomacho, Protogene, Apelle, jam perfecta sunt omnia." Cicero, Brutus, seu de claris oratoribus, c. 18. Pliny himself describes with enthusiasm the Venus ἀναδυομένη of Apelles: and in this picture the sea was represented, which required azure.

The great Greek painters, like the most illustrious artists of the Roman and Venetian school, were probably, however, sparing in the use of the more florid tints in historical and moral painting, and produced their effects rather by the contrasts of colouring in those parts of the picture where a deep and uniform tint might be used, than by brilliant drapery.

If red and yellow ochres, blacks and whites, were the colours most employed by Protogenes and Apelles, so they are likewise the colours most employed by Raphael and Titian in their best style. The St. John and the Venus, in the tribune of the Gallery at Florence, offer striking examples of pictures in which all the deeper tints are evidently produced by red and yellow ochres, and carbonaceous substances.

As far as colours are concerned, these works are prepared for that immortality which they deserve; but unfortunately the oil and the canvass are vegetable materials, and liable to decomposition, and the last is less durable than even the wood on which the Greek artists painted their celebrated pictures.

It is unfortunate that the materials for receiving those works which are worthy of passing down to posterity as eternal monuments of genius, taste, and industry, are not imperishable mar-

* Some excellent pictures have suffered very much in modern times from the same cause; the lakes in the frescos of the Vatican have lost much of the brilliancy which they must have possessed originally. The blues in many pictures of Paul Veronese are become muddy.

† Lib. xxxv. c. 32.

ble

ble* or stone: and that frits, or unalterable metallic combinations, have not been the only pigments employed by great artists; and that their varnishes have not been sought for amongst the transparent combinations of the earths with water, or amongst the crystalline transparent compounds unalterable in the atmosphere†.

Rome, January 14, 1815.

LXXIII. *Experiments tending to prove that the Prism has a calorific Focus, and that Dr.* HERSCHEL *was mistaken in supposing he separated the Heat and Light of the Solar Rays. By* JOSEPH READE, *M.D.*

To Mr. Tilloch.

SIR,—HAVING fixed a piece of pasteboard in the frame of a dressing-glass, in which I cut an opening or slit a little larger than the bulb of a thermometer, and of a sufficient length to let the whole extent of one of the prismatic colours pass through; and having placed a very sensible mercurial thermometer on an inclined plane of wood covered with white paper,—I placed this plane behind the dressing frame sustaining the pasteboard on a table: I now set a prism moveable on its axis in the upper part of an open window at right angles to the solar ray, and turned it about till its refracted coloured spectrum became stationary on the table placed at a proper distance from the window. The experiment was so regulated as to let the rays of one colour pass through the opening in the pasteboard and fall on the thermometer, which when placed in the shade behind the screen stood at 50°.

Exp. 1.—Having arranged the thermometer, and allowed the red, or more correctly speaking the orange rays to fall on the bulb; in five minutes it rose from 50 to 58, or eight degrees.

Exp. 2.—I lowered my thermometer to 50°, by plungeing it in some dry sand at that temperature, and then replaced it on the inclined plane: on bringing the green rays of the spectrum through the opening, so as to immerse the entire thermometer

* Copper, it is evident, from the specimens in the ruins of Pompeii, is a very perishable material, and therefore, even enamels made on copper will yield to time. Canvass, by being impregnated with bitumen, is rendered much more durable, as is evident from the duration of the linen impregnated with bitumen and asphaltum used for infolding the Egyptian mummies.

† The artificial hydrat of alumina will probably be found to be a substance of this kind: possibly the solution of boracic acid in alcohol will form a varnish.—The solution of sulphur in alcohol is likewise worthy of an experiment. Many other similar combinations might be named.

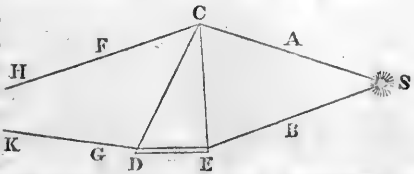
as

as in the former experiment, it rose from 50 to 54, or gained four degrees.

Exp. 3.—Having again reduced my thermometer to 50°, I placed it on the inclined plane; and having allowed the violet rays to fall on it, the rise was from 50 to 51, or one degree.

So far these experiments, assimilated as much as occasion required to Dr. Herschel's, would seem to confirm the inferences drawn by that celebrated astronomer. However, so early as the year 1802, when at Edinburgh College attending Dr. Hope's interesting and scientific lectures on chemistry, I was led to doubt the conclusions drawn from these experiments, but at that early period did not venture to oppose such high authority.

Exp. 4.—Having finished the former experiments, I removed the pasteboard screen, and in its place interposed a sheet of white paper on which the spectrum was received; it was gradually approximated to within one inch of the prism, where it was properly fastened on a stand. The spectrum at this distance was one-fourth of an inch in breadth, bounded on the one side by orange and yellow rays, and on the other by blue and violet, while light occupied the middle; this white light was much more powerful and luminous than the surrounding sun-beams. Indeed, it appeared exactly like the light concentrated in the focus of a burning-glass, which made me strongly suspect that it was likewise more calorific. Nor was I mistaken; for, on placing the thermometer in those rays, it rose in ten minutes to 81°; the heat of the neighbouring sun-shine being only 70°. The thermometer rose and fell alternately as it was removed in and out of the spectrum. From this experiment we must infer that Dr. Herschel made his experiments not only on the heat of the solar ray, but likewise on the heat accumulated by the converging power of the prism. Indeed, it appears rather singular that the action of this well-known instrument should heretofore have been so superficially investigated. Every glass wedge must produce a focal heat, for those rays in the vicinity of the angle must suffer a more powerful refraction than the others; consequently at a given distance the heat and light must be condensed. Let us suppose the vertical section of a triangular glass wedge or prism, resting on its base, to be represented in the following diagram:



AB, two rays of light, coming from the sun S, strike on the prism CDE. The ray B near the base is but slightly refracted, whilst on the other hand the ray A coming through the upper refracting angle is considerably

 bent

bent towards a perpendicular. The letters HK represent the prismatic focus: for as the quantity of heat and light contained in the space FG is by the refracting power of the prism conveyed into the smaller space HK; consequently HK must be more luminous and calorific than FG. In my next paper I shall endeavour to show why the red rays are more calorific than the green or violet, &c.

Exp. 5.—June 23, 1814, at half past two in the afternoon, the sun shining through an open window fronting the west, I placed a highly sensible and correct thermometer (made by Mr. Bath of the Cork Institution) on a sheet of white paper, resting horizontally on a writing-table. In five minutes it rose in the full sun-beams to $82\frac{1}{2}$; and after remaining for some time stationary, I held a large prism at about one inch distance immediately over it, and in such a manner as to convey the rays of light as much as possible to the prismatic focus, and also to immerse the entire thermometric bulb and cylinder in a spectrum of transmitted light. I need scarcely again remark that this spectrum was composed of medion white light, bounded on the one side by orange and yellow, and on the other by blue and violet. In five minutes the thermometer rose to 101; or, in other words, the transmitted light was heated $18\frac{1}{2}°$ above the full sunshine by passing through the prism. On removing the prism the thermometer fell in five minutes to 85°; and on again immersing it in the spectric light it rose to 101°. About half past three I ended these experiments, simple in their manipulation, and obvious in their inferences.

Sir, I beg leave to remain
Your obedient servant,

Cork, May 4, 1815. JOSEPH READE, M.D.

LXXIV. *New Outlines of Chemical Philosophy.* *By* EZ. WALKER, *Esq. of Lynn, Norfolk.*

[Continued from p. 21.]

FROM the discordant opinions which prevail among the writers on electricity, it appears that this branch of physics is still very far from being clearly understood. It has been generally supposed, that where no spark is elicited, no permanent effect is produced upon an insulated conducting body.

But the truth is, that most of the operations of nature, in which electricity is concerned, are produced by *inducted electricity;* and consequently, the effects of those elements on matter become an interesting subject of philosophical discussion.

In order to investigate the laws of electrical phænomena, with that

that experimental precision upon which it is necessary to build a theory, I constructed the following simple apparatus:

1. In fig. 4, Plate VIII. *a*, *b*, *c* is a glass rod of green bottle-glass 2-10ths of an inch in diameter, and eight or ten inches in length, bent to a right angle at *b*. The end *c* is fixed into a wooden foot or base EF, so that the part *b c* may stand in a perpendicular direction. A small perforation is made by means of the blow-pipe, through the end of the glass rod at (*a*), into which a piece of piano-forte wire is fixed, that stands about an inch and a half above the rod, and reaches three or four inches below it. To the lower end of this wire two slips of Dutch gold-leaf are fixed with gum water. I have tried both gold and silver leaf for this purpose; but Dutch gold-leaf, commonly called Dutch metal, an article manufactured out of copper, is far preferable.

The glass jar *w*, *x*, *y*, *z*, three inches in diameter and seven inches deep, is fixed into the base EF. The slips of Dutch gold-leaf are each one inch in length, consequently their ends cannot reach the sides of the glass nearer than half an inch when the wire is in the axis of the glass. The only use of this jar is to defend the slips of metal from being agitated by the air, and from the action of the small particles of dust and moisture that are constantly floating in it.

As green bottle-glass contains no metal, I suppose that it insulates better than white flint, which contains much lead.

2. Another electrometer is made thus: The glass rod *a*, *b*, *c*, being taken away, a stopper of sealing-wax is fitted into the neck of the jar, through which a piece of piano-forte wire passes into the glass, and extends down its axis about one-third of the distance between the stopper and the bottom *y z*. To the lower end of this wire two slips of Dutch leaf are fixed in the manner above described.

This electrometer is more convenient for some experiments than the other, and less liable to accidents.

I have various other forms of this instrument, which need not be described in this paper.

3. Several insulating stands, each consisting of a glass rod ten inches in length and 2-10ths of an inch in diameter, standing upon a brass foot. These have each a circular piece of plate glass two inches and 3-4ths in diameter, fixed upon its top with sealing-wax. The upper surface of this glass is gilt with gold-leaf, and upon its centre a slip of Dutch leaf about an inch in length is fixed by one of its ends.

4. A brass ball two inches in diameter, having a slip of Dutch leaf fixed by one of its ends to the top of it, is supported by a brass wire fixed into a brass foot.

5. An

5. An electrometer constructed according to Mr. Bennet's directions, and some other instruments used for the same purpose. They are very ingenious inventions, but improperly called electrometers; they are electroscopes, which indicate the existence of electricity, but not the quantity of it.

From the following experiments it will appear that some of the principal hypotheses which have been invented to explain the phænomena of electricity are erroneous.

Exp. 1. A barometer tube 4-10ths of an inch external diameter being excited by rubbing it with silk, and held over the top of the wire of one of the electrometers above described, for a short time, at the distance of six, nine, or twelve inches, according to the degree of excitement, or state of the air, the gold-leaves will diverge to an angle between 90 and 180 degrees; and if the instrument be a good one, this angle will gradually decrease during two or three days; but these instruments in general will not retain electricity quite so long.

Exp. 2. The barometer-tube being excited as before, and held over one of the insulating stands with a gilt surface, at the distance of three or four inches, for a short time, the narrow slip of Dutch leaf stood erect for near two hours. At the end of that time, the quantity of electricity which the gilt surface contained was measured thus: the wire at the top of an electrometer being brought into contact with the edge of the gilt surface of the glass, the leaves of the electrometer diverged, and the angle being measured was found to contain 50 degrees. The electrometer being discharged, by touching the wire with a finger, and applied a second time to the gilt surface, the leaves diverged, and the angle being measured was found to contain 30 degrees. Thus the remaining quantity of electricity was measured, and the sum of the angles indicated by the electrometer amounted to 110 degrees.

The divergency of the leaves is measured by a graduated arc placed against the outside of the jar, with its centre just as high above the table on which the instrument stands, as the angular point of the two leaves.

But the two electrometers described in this paper, will not indicate the same number of degrees in a given quantity of electricity, because their leaves are of unequal dimensions.

Exp. 3. After one of the insulating stands A had been electrified by induction, as in the last experiment, I took it up by its foot and held it in an inverted position over the top of another stand B for a little time; the distance between their two surfaces being about three inches. The slip of Dutch leaf upon B, being attracted by that on A, stood erect, but in a short time the leaves ceased to indicate any electrical signs; for the electricity

tricity of A was equally divided between A and B, which became known by measuring them with an electrometer.

Exp. 4. The gilt surface of the insulating stand A being electrified by induction, as before, and held over the brass ball No. 4 communicating with the earth, the slip of gold-leaf upon it stood erect, pointing to that on A, and strongly attracting each other: hence I concluded, that the two surfaces were in contrary electrical states. To demonstrate the truth of this supposition, I excited a stick of sealing-wax by rubbing it with a woollen cloth, and held it at a proper distance from the two slips of metal: that upon the gilt surface was attracted by it, but the other was repelled.

As a further proof of this test, I held one of my electrometers, so that the wire upon the top of it came into contact with the slip of metal upon the brass ball. The leaves of the electrometer received photogen (minus electricity) and diverged to an angle of 40 degrees. The electrometer being discharged, and applied a second time to the slip of metal upon the brass ball, the leaves of the electrometer diverged with the same element as before: thus, by continuing the operation, electricity may be drawn from the same object as long as the gilt surface of the insulating stand contains power to attract it from the earth. When the gilt surface of the insulating stand is charged with thermogen, it attracts photogen from the earth; but when the gilt surface is charged with photogen, thermogen is drawn from it. These experiments are delicate, and will not succeed in all states of the earth, unless the electrometer be a very good one. But this is a subject which remains for further investigation.

The attraction between the two elements was so strong, that when the slip of metal upon the brass ball was bent out of its perpendicular direction, by the wire of the electrometer, it instantly returned into its former position, as soon as it was set at liberty, like an elastic fibre.

This element certainly came from the earth; for, when an insulated glass ball was used in the same manner as the brass one, the slip of metal upon the glass soon became electrified, but in the *same state* as that above it; for, all communication being cut off between the earth and the glass ball, it could not receive electricity from any other object.

Whence it is evident, that when the inside of a Leyden jar is receiving thermogen from the electric machine, this element attracts photogen to the outside from the earth, and it is only the coating that prevents their union; for, take away the coating, and the jar will no longer contain those elements, because they pass through glass like light or magnetism.—This property of electricity admits of an experimental demonstration.

This

This experiment explains in a very satisfactory manner some of the grandest phænomena of Nature.

If the gilt surface electrified in the manner above described represents a cloud highly charged with electricity, and the brass ball the earth, then it appears evidently, that the cloud attracts the contrary element out of the earth, at the same instant that the earth attracts that contained in the cloud; and these two elements of combustion, passing through each other in contrary directions, generate lightning.

This experiment also explains the cause of water-spouts. A cloud being charged with one of the elements of electricity, attracts not only the contrary element out of the sea, but has sufficient power to lift a portion of the water also, which at the same time conducts one of the elements from the cloud to the sea, and the other from the sea to the cloud. And the moving pillars of sand, seen by Mr. Bruce and other travellers, upon the desert plains of Africa, are raised by the same two elements.

It may, however, be deemed impossible that any power in the clouds should be capable of raising up such heavy masses of matter as those above mentioned; but when we compare the effects of a barometer-tube of only 4-10ths of an inch in diameter, with the effects produced by a cloud of some miles in area, this objection must vanish. Indeed, the effects seen in our experiments are matters of astonishment quite as great, when placed in a philosophic point of view, as those produced by a thunder cloud. The efficient causes are the same in both cases, and equally unknown to us.

Bacon and Newton showed the necessity of building philosophical theories upon geometrical and experimental principles: but these tedious modes of investigation are frequently avoided, by building theories upon some supposed principle, or erroneous experiment made with imperfect instruments. Hence much learning and ingenuity have been exercised in building an aërial fabric, which vanishes like a shadow seen through a transparency, as soon as the novelty is over, or on the first appearance of an experimental demonstration.

It has been a disputed point ever since electricity was brought into the form of a science, whether glass is permeable by the electrical fluid. This uncertainty has not been owing to the want of ability or attention in those who cultivated the science, but probably to the imperfection of the instruments which they used in making the experiments.

Dr. Franklin and his friends concluded from their experiments, that glass is impermeable by the electric fluid; and this hypothesis seems to have met with general approbation.

In the year 1780, the Rev. John Lyon, of Dover, published some

some experiments and observations on electricity, in which he attempted to prove that glass is permeable to the electric fluid. The gentleman who wrote a critique on that work in the Monthly Review, being a Franklinian, objected to the experiments which related to the permeability of glass, in a manner which did not exactly accord with the author's ideas of the subject. In the year following, Mr. Lyon published some "further proofs that glass is permeable by the electric effluvia." The same gentleman who reviewed the former work reviewed this also; and some drops of ink were spilt between the two disputants, which might have been spared without doing much harm to the subject.

I do not remember to have seen, since that time, any attempt to subvert the Franklinian theory (as it is called), till I read the papers of Mr. Donovan and M. De Luc in the present volume. Mr. Donovan observes "that glass is, in the strict sense of the word, permeable to electricity, yet the fluid passes through it with so much difficulty and so slowly that Franklin's position might be admitted."

In the first observation Mr. Donovan is certainly correct; but as to the time of its passing, this learned philosopher seems to have been deceived, by his having made use of coated glass.

My experiments on this subject prove, not only that glass is permeable by the electrical elements, but that they pass through it instantaneously.

Exp. 5. I took a pint decanter with a glass stopper ground to fit into it very exactly, and to one end of a piece of thermometer-tube two slips of Dutch gold-leaf were fixed with gum-water; the other end being fixed with sealing-wax to that part of the stopper which goes into the decanter. This tube being suspended in the axis of the glass, its mouth was closed by the stopper as perfectly as could be done with glass, without the use of the blow-pipe.

The decanter being suspended by its neck, and an excited barometer-tube caused to vibrate under it, at the distance of two or three inches, the gold-leaves vibrated nearly as much as they would have done had the glass been open at the bottom.

From a great number of experiments made with this apparatus, I concluded that the electrical elements pass through glass instantaneously. But it may be said that the electrical element went into the glass between its neck and the stopper. This is not impossible, though not probable; for the element must have first passed over the outside of the glass from the bottom to the top, and then from the top to the bottom on the inside, a distance no less than seventeen inches. However, it must be admitted that *no demonstration contains a single step that is in the least doubtful.* To demonstrate in the most satisfactory manner, that

that the electrical elements pass through glass instantaneously, I ordered a glass ball three inches diameter to be blown with a long neck, that a thermometer-tube might be sealed into it with the blow-pipe. But as the artist to whom I gave the order did not execute it so soon as he might have done, he gave me an opportunity of trying what might be done without his assistance.

It is well known to every tyro in this science, that animal substances are better conductors of electricity than glass. On this principle I supposed that the electrical elements might be prevented from entering into a glass phial, as effectually as if its mouth were hermetically sealed.

To try what might be done on this principle, I procured a Florence flask, and fitted into the top of its neck a glass stopper, but not by grinding, for that was not necessary in this experiment. To the lower part of the glass stopper, one end of a strip of Dutch plate-glass, a quarter of an inch in breadth, was fixed with sealing-wax; to the other end two slips of Dutch leaf, each an inch and a half in length and a quarter of an inch in breadth, were fixed. The strip of glass being suspended in the axis of the flask, its neck and stopper were covered with moist bladder.

The palm of the hand being laid upon the stopper, and an excited barometer-tube carried over the back of it with a vibrating motion, not the *least effect* was produced in the leaves within the flask; for all the electricity communicated to the hand was conducted to the floor, through the person of the experimenter*.

But the flask being taken up by its neck and an excited tube of glass held under the bottom of it, at the distance of two or three inches, the leaves instantly diverged; and when the tube was caused to vibrate horizontally, even to the number of four or five times in a second, the same number of vibrations were performed, in the same time, by the leaves within the flask: and he same phænomena were produced by a rubbed stick of sealing-wax, though in a less degree.

These experiments demonstrate in the most satisfactory manner, that glass is permeable by the electrical elements, and that ney pass through it instantaneously, both by induction and dièct communication.

Exp. 6. A brass ball properly insulated will become electrified by induction, as soon as a charged barometer-tube is held over it at a proper distance, but in a state of electricity contrary to that of the tube.

* When the top of the flask is perfectly covered with the hand, the bladder is unnecessary.

The

The ball, in its natural state, contains the two elements of electricity, thermogen and photogen, diffused over its surface: consequently, if the tube be charged with thermogen (positive electricity) it will repel that element from the surface of the ball, because elements of the same kind repel each other; but the other element, photogen, will remain upon the surface of the ball undisturbed.

To prove the truth of this theory by experiment, I placed an electrometer with the top of its wire in contact with the underside of the ball; and a barometer-tube, rubbed with silk, being brought so near the ball as to electrify it by induction, the leaves of the electrometer diverged, containing thermogen.

This electrometer and the excited tube were taken away from the ball, and another electrometer being applied to it became electrified, but in a state contrary to the former.

Thus, the two electrometers became permanently electrified, by the two elements which the ball possessed in its natural state. The first electrometer received a portion of thermogen which was repelled from the ball by the excited tube, but the other was electrified by the photogen which remained upon the ball after the tube had been removed.

Exp. 7. Two Dutch-leaf electrometers, being placed upon a table at the distance of about an inch asunder, and an excited barometer-tube held over them at the distance of about two inches from each, became permanently electrified. One of them received the same element as the excited surface, but the other received the contrary element.

Explanation.

All the metal contained in one of these electrometers, No. 1, weighs only three grains, and consequently the quantity of electricity which it contains in its natural state may be deemed almost nothing; wherefore electrometers of this construction *always* receive that element to which they are exposed, whether they receive it by induction or direct communication*.

But the other electrometer, No. 2, had a brass ball of an inch in diameter fixed upon the top of it; and as soon as the excited tube was brought near it, the two elements which were diffused over its surface, in its natural state, began to be separated. The

* If this electrometer be brought into a room containing an atmosphere of thermogen (positive electricity), its leaves will diverge with that element to an angle containing between 90 and 180 degrees. Then, if it be carried into another room, some time will elapse before any diminution in the angle contained by the leaves can be perceived; but this time will depend upon the perfection of the instrument.

element

element of the same kind as that of the excited surface was repelled into the air; but the other remained upon the surface of the ball. The tube being removed, the electrometer, No. 2, became permanently electrified, but in a state contrary to that of No. 1.

From the two different electrical states of the electrometers, with and without the brass ball, it will be easily understood, that there is a certain construction of the instrument which will not remain permanently electrified. When the surface of the ball is equal to the surfaces of the two leaves, all electrical signs will vanish as soon as the excited surface is removed.

Exp. 8. From some papers lately published in this Magazine, it appears that the properties of the pith-ball electrometer are not clearly understood. To satisfy myself in this particular, I laid a thermometer-tube upon an insulating stand in a horizontal position, and hung a pith-ball electrometer upon it near one of its ends. These balls being immersed in an atmosphere of thermogen diverged with that element.

But when I fixed a brass ball upon one end of the glass tube, with a pith-ball electrometer attached to it, and immersed the balls in an atmosphere of thermogen, they diverged, but in a contrary electrical state, for the reasons given in the two last experiments.

Lynn, May 17, 1815. EZ. WALKER.

[To be continued.]

LXXV. *On an ebbing and flowing Stream discovered by boring in the Harbour of Bridlington. By* JOHN STORER, *M.D. Communicated by the Right Hon. Sir* JOSEPH BANKS, *Bart. K.B. P.R.S.**

THE following account of certain peculiarities attending a spring of fresh water, which was tapped in boring within the harbour of Bridlington quay, Yorkshire, is given from repeated observations made during a residence of some weeks there, in the months of July and August 1814. The harbour of Bridlington quay is dry at low water, except for a rivulet which traverses its bed: at high water, it has from fifteen to seventeen feet of water.

Mr. Rennie, civil-engineer, was consulted in the year 1811, respecting certain improvements projected in that harbour. At his desire, with a view to ascertain the depth of the stratum of clay in the harbour, the boring, which terminated in forming the

* From the Philosophical Transactions for 1815, part i.

well

well to be described, was begun under the direction of Mr. Milne, collector of the customs for the port. The spot fixed upon is opposite to the termination of a street leading to the harbour, and has about six feet of water, at high water, in ordinary tides.

After the workmen had bored through twenty-eight feet of very solid clay, and afterwards through fifteen feet of a cretaceous flinty gravel, of a very concrete texture, the auger was perceived to strike against the solid rock; but as they were not able to make any impression upon it, the work was given up for that tide, without any appearance of water from the first. In an hour or two afterwards, the bore was found filled to the top with fresh water, of the most limpid appearance: it soon flowed over, and was even projected some inches above the summit of the bore, in a stream equal to its calibre. When it was ascertained that the water was of the purest quality and taste, perfectly fit for washing, and every culinary purpose, the bore was properly secured by an elm stock, ten feet long, and perforated with a three-inch auger, driven to its full length: a copper tube, well tinned on both sides, of a circumference to admit its being passed through the bore of elm stock, and thirty-two feet in length, was then forced to the bottom of the bore, so as to rest on the rock. The upper part being properly puddled round the elm stock, and the well thus completed, the following singular circumstances were observed, and have continued with great uniformity ever since.

As soon as the surface of the sea water in the harbour, during the flowing tide, has arrived at a level of forty-nine or fifty inches lower than the top of the bore, the water begins to flow from it in a stream equal to its calibre, the impetus of which is increased as the tide advances, and may be observed to be propelled with much force after the bore is overflowed by the tide. The discharge continues from four to five hours, *i. e.* till the tide in returning falls to the same level where it began to flow: at this point, it ceases completely till the next flood shall have regained the same level, when the same phænomena recur, in the same succession, and without any variation, but what arises from the different degrees of elevation in the tides. The rule appears to be, that the column of spring water in the bore is always supported at a height of forty-nine or fifty inches above the level of the tide, at any given time. This at least was the result of every observation I made during several successive weeks, in the months of July and August last; and I am assured by Mr. Milne, on whose ingenuity and habit of accurate observation I can place the firmest reliance, that his habitual experience, for three years

 past,

past, goes to convince him, that the variations from the rule stated above, are very inconsiderable during the summer and autumnal months; but that in winter, after any unusual fall of rain, he has known the column of fresh water raised eight feet above the level of the tide, and the period of its discharge proportionally prolonged.

For the use of the town and shipping, a reservoir of brick-work, capable of containing one thousand gallons, has been constructed within two or three yards, and upon somewhat a higher level than the summit of the bore, and is made to communicate with it by a tube of the same diameter, fitted with a valve, to prevent any reflux into the well. Two waste pipes are placed within a foot of the top of the reservoir, for the regular discharge of the water, and it has also been made to communicate with a pump adjoining, by which the reservoir may be emptied; and as the bore of the well is now closed and secured at the top, it is obvious that the commencement of the flow of water, from the pipes of the reservoir, will happen a few minutes sooner or later at each tide, according to the quantity of water it contained at the time. Such, however, is the known regularity of the discharge from the waste pipes, that at the expected time of the tide several of the inhabitants are always on the spot with their vessels, and are rarely obliged to wait for more than five minutes.

Such is the state of facts, and it appears to open a subject of curious investigation to those whose habits and practical knowledge qualify them for it. The appearances seem not to admit of any satisfactory explanation, without supposing some mode of subterranean communication, by which the water of the sea, and that of the spring in question, are brought into actual contact, so as to exert a reciprocal action. This supposition receives considerable support from a circumstance which I had no opportunity to observe, but which Mr. Milne has had frequent occasion to notice; and which he describes by remarking, that after stormy weather, when there is a heavy sea on that coast, the water is discharged, even from the waste pipes of the reservoir, with an evident undulation; which, of course, would be more considerable from the original bore.

Mr. Milne has framed an hypothesis to satisfy his own mind on this curious subject. He believes the stratum of clay found in the harbour, to extend over the whole bay in front of it, as far as the Smithwick sand, which forms a bar across the opening of the bay, in a direction from Flamborough head towards the Spurn point, and about four miles from the quay in a south-easterly direction. This bank is supported by a reef of rock; and

and though there are openings, which are well known, and admit vessels of considerable burthen at all times of the tide, there is in general but a small draft of water on this bank, when the tide is out. On the outward or east side, towards the ocean, the rock is quite perpendicular, and a great depth of water is immediately behind it. As the copious source of water, which has been tapped in the harbour, lies at such a depth, and under a stratum of clay, there is no reason to think that it can be discharged any where in the bay, till it arrives at the ledge of rock where the clay terminates. Here, among the fissures of the rock, it may find its exit; and this is the more likely, as it is known that the bed of the sea at the back of the Smithwick sand is at so much a lower level.

Admitting this supposition to be correct, or nearly so, it seems to follow, that the issue of a body of fresh water, through a fissure in rock forming the bed of the sea, would meet with more or less resistance at different times of the tide; because the two columns of fluid in meeting, would act upon one another in the ratio of the altitude of each, taking into the account the difference of their specific gravity; and thus, if there is any approach to an equilibrium, an operation would result, analogous to the flux and reflux of the tide, near the mouth of rivers.

This hypothesis is specious, and accounts for the flux and reflux of the water from the bore, as well as for the singular undulation of the discharge in a boisterous state of the sea: but the greater relative altitude to which the column of spring water is elevated after much rain, and the consequent prolonged discharge of it during each tide, seems to militate against its correctness; since, in a case, where by the supposition a balance is nearly established, an additional impetus communicated to the column of spring water, ought to produce the opposite effect, by enabling it to overcome the resistance of the same column of sea-water during a longer period of each tide, than under the usual circumstances.

It is not improbable, that this whole subject might be elucidated, by a more perfect acquaintance with the peculiarities of the springs on this part of the coast, provincially termed *gipsies*. The water in this district of the east riding of Yorkshire possesses that limpidness which is usual in cretaceous soils; but for many miles of the Wolds behind Bridlington, very little water is to be seen. There are few rivulets, and these are very low in the summer, and most of them quite dry in autumn. The account to be collected from the inhabitants is, that in two or three weeks after the commencement of frost, the springs begin to run copiously; and in many, the water is projected with such impetuo-

 sity,

sity, as to resemble a *jet d'eau;* it is then that, in the language of the country, it is said, "the gipsies are up," and the rivulets overflow.

Nottingham, Nov. 5, 1814. JOHN STORER.

LXXVI. *On certain Accidents to which Coal-works are liable, particularly those of Water bursting into the Pits from old Works that are near adjacent, as recently occurred at Heaton in Northumberland: the accidental Explosions of Fire-damp, setting Fire to the waste Coals in the Works, as happened last Summer at Brora in Sutherland; and the spontaneous Firing of loose small Coals and pyritic Dirt, &c.* By JOHN FAREY, *Sen., Mineral Surveyor.*

To Mr. Tilloch.

SIR,—THE vast importance both to their owners and the public, of the extensive and curious Works which are carried on underground in these Kingdoms for procuring that truly essential article fossil *Coal,* seems to have failed of attracting such a general attention to them, and to the principles on which they are or may be best and most securely conducted, as their importance has demanded; until of late, that the sympathy and feelings of a portion of the public has been roused, by the lamented losses of Lives that have occurred. Although on one hand, the rights of private property, and the respect due to characters of the first respectability who are Owners and Lessees of Coal-works, to the professional talents and to the private characters of the Agents, Over-lookers, and Men employed, call for and require the utmost delicacy, in speaking or writing for the public Eye, on their individual concerns or proceedings; yet, on the other hand, in a matter of so much importance as the preventing of the distressing catastrophes which have of late years wrung the hearts of the Inhabitants of Durham and Northumberland, it may appear little short of criminal apathy, in those who may happen to have seen much of the management of Collieries in this or other districts, not to endeavour, by as plain and intelligible descriptions as possible, to make the true circumstances of the unfortunate cases fully known to the public, and in temperate and proper terms to describe, the defects of system or management that they may perceive therein, in order that the influence and weight of opinion, of persons conversant with the subject, and of the intelligible part of the public, may be brought in aid of the recommendations that may be made, of an improved system or management

ment of these important Concerns: and without which aids, the representations or volunteer suggestions of persons unknown to most of the parties, could be expected to have little attention given them.

With these views I would beg to occupy the necessary pages in your very useful miscellany, for describing the leading circumstances of *Heaton* Colliery, from the materials kindly furnished by your Correspondent N. at page 364, and from other sources, and which will be best done, I conceive, by beginning with a clear idea of the situation of the *seam* or stratum of Coal which is *wrought* or dug in this Colliery, where the same formerly approached near to the surface.

The Tyne River has its course nearly from W to E, where it passes close on the S side of Newcastle Town, between it and Gateshead; about ¾ of a mile below the Bridge of Newcastle, a small stream or rivulet falls into the Tyne, from the North:—if from the Tyne we go northward up the bottom of the sudden valley or *burn* in which this rivulet runs, we shall pass Useburn on the N Shields Road, and leave Heaton-Hall and its grounds (the seat of Sir Matthew White Ridley, Bart.) on the rising ground E of this Burn, at about 1¼ m. distant from the Tyne: and along the surface in the bottom of this Burn, or at no great distance below it, all the length, the Heaton seam of coals, about six feet thick, may be conceived by the Reader, to range, nearly level in a S and N direction, but the plane of which Coal-seam (imbedded between two hard argillaceous stoney strata or *beds*, called its *roof* and its *floor*) has a considerable declension or *dip* to the eastward, causing it to descend deeper and deeper, the further it may be followed in that direction, under other strata and Rocks*, dipping in like manner to the eastward.

The first Colliery mentioned in this Estate, was wrought many years ago, and was called *Heaton-Bank* Colliery, its deep or Engine-pits were each situated so far to the eastward of this Burn, that the Coal was not reached in these perpendicular pits until near 110 yards deep:—according to the usual practice of Colliery-ing, two parallel and level passages or *Gates* were excavated in the Coal, in both N and S directions, from the bottoms of the Engine-pits wherein the pumps were situated for draining the works; and by means of which Gates, and others branching from

* By consulting pages 145 and 146 of Mr. Westgarth Forster's "Treatise on a Section of the Strata," published at Newcastle, or Plate II in your present volume, and conceiving about 15 yards of strata surmounted by alluvial Rubbish, to be added on the top of the "Brown Post" or Newcastle *Grindstone Rock*, mentioned at the beginning of his Section, instead of the 10 yards of "Clay and Soil" there shown, the Reader may obtain a pretty accurate idea of the number and succession of the strata in the deep Pit of *Heaton* Colliery, wherein the "High main Coal, on Tyne" is wrought

them W, as will be further described respecting the new or present Colliery, all the Coal lieing up the slope or *rise* of the stratum to the W, from these Level Gates, was wrought out, almost to the *basset* or near appearance of the stratum of Coal on the surface in the Burn, I believe, as already described.

The reason of Engine-pits being mentioned above, arises, from a vertical fissure or crack of some width, breaking through all the strata from the surface to a great and unknown depth (and ranging SW and NE, I believe) called a *Fault* or Dike, which divided this old Colliery into two parts; in the northern part of which, the Coal-seam and all the strata above and below it, were found raised or thrown up, with respect to those in the southern part, 16 yards in height, and this fault or natural barrier, solidly filled with *water-tight* clay and earth called *Fault-stuff*, the old Colliers were careful never to penetrate, so as to connect the waters in the underground works of these northern and southern parts of the old Colliery.

On the abandonment of the old Colliery, it should seem, that the vertical pits of the northern part were closed or filled up with earth or rubbish, but some of those of the southern part, in front of Heaton-Hall, were strongly floored or *scaffolded over*, at 10 yards below the surface, and the pit only filled up with Earth above this flooring, leaving the lower part open to the *hollows* or excavated chambers, whence all the Coal had been taken, that it was judged safe to remove, without letting down the roof on the heads of the workmen. And by the soakage of rain-water through the strata (and by their edges) from the surface, these old hollows soon became quite full of water, resting against the unwrought or *whole Coal* of the seam, below the Level Gates, on all the E or deep side of this deserted Colliery, rising several yards up the open pits, and thereby giving a *hydrostatic pressure,* equal to this column of water in the vertical pits, to great part of the body of water in these Coal-hollows.

On the opening of the new or Heaton Colliery in 1790, a situation was chosen for sinking the Engine-pit, so much further to the E, or *in the deep* of the former Engine-pits of Heaton-Bank Colliery, that the Coal-seam was not reached therein till the depth of 164 yards; and in order to avoid the expense of sinking two Pits, one for the pumping and the *up-cast* or ascending current of light and contaminated air, and another for the drawing of Coals and the *down-cast* or descending column of fresh air (as would in most other districts of Britain have been done, where the Coals lay *at no greater depth* than here) one wide pit, separated by a boarded partition from top to bottom, was made to answer both these essential purposes.

From the bottom of this Heaton Pit, two parallel level Gates were

were driven out N in the Coal, the upper or W one for carrying out the fresh Air, and dragging the Coals to the Pit bottom, the other for returning the heated Air, and bringing the Water to the Pumps*; and a similar pair of Gates having been extended in the Coal S from the bottom of the Pit, the *workings*, by means of long chambers, galleries or *board-ways* therefrom, up the slope or plane of the Coal-seam, and leaving intervening pillars thereof, has ever since been going on, by means of this *one vertical Pit !*.

In this Pit, ample power of Engines and Pumps had been provided, for lifting to the surface (or to the Day-Level or drain, which naturally discharges the water) all the water which it was calculated would soak through the whole breadth of strata (or enter by their exposed edges) covering the present Colliery, and the two parts of the old or Heaton-Bank Colliery, *in the rise* of it: and it fortunately happened, that when the Boards or *working Gates* first proceeded near to the Level Gates of the southern division of the former Colliery to the W, its vast subterranean reservoir of water, was quietly and by degrees discharged into the new works, so as not to drown them or over-power the Engines: but the large body of water in the northern division of the former Colliery still remained penned up, and threatening sudden destruction to the men, on cutting through the Fault and approaching the old Coal-hollows beyond it.

It has already been hinted by a Correspondent, in your 117th page, that *too few pits are sunk*, in many of the Collieries in this district, to prevent the dangerous accumulation of inflammable gas; and the same conclusion must, I think, occur to every reader of the above description, and particularly as to the want of an Air-pit (or even more than one perhaps) at the western extremity of the new Colliery at Heaton, where the depth of the seam is stated not to exceed 110 yards beneath the surface; and through which the unfortunate Men and Boys now lost to Society, might with certainty have escaped !. I would not be supposed to maintain, that this defect of management, in having too few vertical Pits, is peculiar to the districts of the Tyne and the Wear, although it would be wrong to deny that it seems more prevalent there as *a system*, than elsewhere; because, I have

* In ordinary cases, where the extent of the works and quantity of hydrogenous and carbonic gases evolved, would not *endanger explosions*, or the air be *too impure for convenient respiration*, it is usual to reverse the arrangements here mentioned (and on which I may perhaps have been misinformed), and cause the fresh Air to descend by the Pumping-pit, and go out therefrom by the Water-Levels; and the same Air, after visiting and ventilating every part of the works, returns by the Level-Gates or *rolly-ways*, and ascends the drawing-pit. See my Derby. Report, vol. i. p. 342.

had occasion, to notice and investigate similar defects in the Colliery management of places, very distant from these districts, and where the lives of the Men have been dreadfully endangered, by long-extended inclined planes of works and hollows, rising up into Grounds, wherein *no pits were sunk* (even where vast reservoirs of water in old works were actually *undermined*), and in which cases, it is next to impossible to preserve from accidents, the artificial current of air, which must be *so many times carried up the slope and down it again*, for airing all the different parts of the works, so that finally this heated and *lighter part of the air*, shall descend to *the lowest part* of the work, in order to enter the bottom of the deep up-cast Pit, to make its escape: nor is it possible to prevent the Men being sometimes shut into the upper works, for a time in hourly dread of being drowned, and afterwards of being suffocated or starved to death !.

The objections or difficulties that I have heard stated or seen with regard to sinking more Pits, have been of three kinds: 1st, the *expense* of such extra pits; 2d, the danger of *letting down water* from the upper strata or *Measures* by each of such Pits; and 3d, the Occupiers of the Land, either on the score of profit or pleasure, *objecting to the breaking of the surface.*

I propose to say something on each of these heads: and,

First, as to the expense of sinking Pits, it is usual in most districts to sink *two* Pits in the deep of the intended works, the Engine-pit and its Bye or *Drawing-pit;* and by help of them to drive two parallel Gates up the slope to the extent of the proposed works, and there to sink an up-cast or air-pit, which always remaining open (except in extreme cases of deficient draft) and being sometimes furnished with ladders, provides the sure or ready means of escape, in case of such a catastrophe as has befallen several Collieries within my knowledge, besides the recent and more severe one at Heaton.

In such very deep works as those near Shields, it certainly is of the utmost importance to the profitableness of the concern, to save the very serious expense of sinking a Pit, whenever it can with safety be omitted, and the making of one Pit serve in *the deep*, when divided by Boarding, for the preliminary operations of running out the Level-Gates and the *Rise-Gates* to the Air-Pit, to be sunk at the top of the works (unless the old Engine or drawing Pit there, can be had for such purpose), and afterwards to serve for the descent of air and for the Pumping and Drawing of Coals, seems allowable:—if I should be told, that the profits of *these very deep Collieries*, would *not pay for such extra Pits*, I should answer, that this can only have arisen, from one or more of four things; viz. 1st, these Pits had been opened *before their*

proper

proper time, while extensive parts of the same or other Coal-seams remained unwrought, in situations where they *might be* raised and conveyed at less expense*; or 2d, the Coal-owners had stipulated for a higher part or share of their Coals, than their depth, and not being yet ripe for market, had entitled them; or 3d, that the Lessees or workers of the Coals, required greater profits than the nature of the adventure they had entered on, entitled them: or 4th, that too great a spirit of rivalry and competition had existed among the workers of Coals in this deep district, in lowering the price of their Coals, so as to have sunk the general profits below their just standard.

It would be unjust in me, to leave what I have said, subject to the interpretation, that I have anywhere found the blame of this niggardliness of sinking Pits, directly or fairly chargeable, in any case, on the Owners or Lessees of the Coal-works I have alluded to, because the facts seem rather to have been, that the *Overseers*, to whose skill and practical knowledge entire deference has been paid, as best understanding what was necessary, and as daily and hourly *sharing with their Men the dangers*, of omitting or neglecting any necessary plan or precaution, have alone made themselves responsible, for the deplored consequences that have followed: and their Superiors have remained ignorant, of the dangers and risks that were unnecessarily run in their works, until apprised of it by the shocking relations, that have come before the public.

And respecting these Superiors, I will not suppose that a British Land or Coal-owner, when properly instructed and advised, on the system of precautions proper to be pursued in his Coal-works, would not as highly and indeed more truly value a Coal-overseer, who should be ready and anxious to prove to him, that *the lives of the Men were never unavoidably risked* in the works under his care, yet with due regard to œconomy in all the plans pursued, than he would another Overseer, who should pride himself, on having *netted as much or more money* from the quantity and nature of the Coals wrought, than any of his brother Overseers, on the same or adjoining Estates, without mention of the comparative security in which this was accomplished; much less would they value or retain one, who should evade this inquiry, or gloss over the cruel facts, by saying, that *no greater*

* I forbear on the present occasion, entering on the unparalleled circumstance, of these vast exporting districts of the Tyne and the Wear, being yet unfurnished with any *public* Canal or Rail-way (where so many of the latter are wanted) for the conveyance of Coals to the ships or the River Barges or *Keels*, and the system of "way leaves" being substituted, whereby so grievous and direct a tax is levied by private individuals, on the Inhabitants of all the east and south coast of England, and even of its south-eastern interior.

risks

risks were run than usual, and such as *Men in plenty could be got to run for the ordinary pay*, and such as *his wages induced him to run !*.

Secondly, with respect to the letting down of Water into the works, by new Pits, it must be admitted, that the upper measures frequently connect with such an extent of porous surface exposed to the rains, or to the beds of Rivers or large waters, that every new pit sunk, with only the ordinary precautions in its walling or lining, would infallibly increase the quantity of water in the Coal-works beneath, almost beyond the expense of Engines, that could, *at the present selling price of Coals*, be employed to pump it out: yet on the other hand, the method of *tight walling* the Pits, in the water-setting or *Lias* Lime, as has long been practised in the vicinity of the Somersetshire Coal Canal, with such perfect success, as to be enabled to extend their Coal-works, that are *perfectly dry*, down the slope from their Pit bottom, to vast depths beneath the surface, some of them to near twice the depth of any Pit near the Tyne, I believe, might be as certainly practised in the latter district, whose upper measures are not more charged with water, than those over the deep works in Somersetshire: and even without this tight-walling, there are numerous instances where the soakage water, in danger of being let down into a deep Colliery by new Pits, might be otherwise drawn off, by soughs or drains, or *be prevented entering the measures*, by attention to the bottoms of the Brooks and Rivulets, and the protecting of the porous rocks or strata from access of such water, by a sufficient covering of clay or water-tight earth, by more attention to *draining the surface*, (see my Derby. Report, i. 351), &c.

The necessity of either dispensing with some of the Pits that would be proper, or of tight-walling such, arises in many instances, from the pen of water in the old works in the *rise* (and in the *range*, in many other instances) *standing in the old Pits against the porous rocks and strata* that are cut through therein, and which must be again cut through in every new Pit, charged with such constant supply of water, to be thereby let down into the deep works, as long as such pens of water in the old works are suffered to remain, which they ought no longer to do, as I shall further mention presently.

Thirdly, as to the almost insuperable objection, which Gentlemen and Farmers have, to permitting Colliers to sink in, or have access to their Parks, Lawns or Farms, whenever it is in their power to prevent it: it must be admitted, that the spoil and disfigurement of the surface, which the Collier generally makes, in a very short time after his commencing operations, and the wide and careless spread which he is too apt in time to give,

to

to his devastations of the Land, have so justly and seriously alarmed may Gentlemen, that they will sooner forego the advantage of working their Coal, than submit to it; while several others whom I know, would on almost any terms buy up the Coal-Lease that themselves or their Fathers had granted, if this were in their power: it is not therefore to be wondered at, that in almost all Coal Leases of modern dates, the exact limits within which the Collier may *at his discretion* commence and pursue his operations from the surface, are exactly defined, and beyond which limits he must *first obtain leave* of the Land-owner, before he can sink Pits, make Roads, &c.

Under such circumstances, I have known an instance of the Overseers going on, to work from a pair of Pits in the deep, great distances up the slope, under the ancient works in upper Coals, the Pits of which were yet open in the Lessees' Park; and although whole gangs or *shifts* of Men were repeatedly and instantly killed by Fire-damp explosions, the Gentleman or his Land Agents, were never once applied to by the Overseers, or their employer the Lessee, who lived at a distance, for permission to open an Air-pit to the higher part of the low Coal, which might have been done, by only deepening one of the numerous pits already open in the Park, with little or no interruption thereto, and which would readily have been granted, and even offered, if it had occurred, or been stated to the Owner, that such a measure was any-way essential, particularly *for saving the lives* of his parishioners and tenants.

If in any instances Gentlemen have refused permissions that may have been asked by their Coal Lessees, to enter restricted grounds, I cannot believe, from anything I have seen or heard, that such refusals have been attributable to any thing else, than the want of a sufficiently clear statement, and representation of the case, showing the importance of the indulgence asked, *for the security of the workmen*, and that the interest of the parties, in cheaper working their Coals or more readily disposing of them, were not the chief if not the only motives for the application, to enlarge the powers of the Lease.

It does not always sufficiently occur to Coal-Lessees and Overseers, that *the occupation of the surface of the Land*, in a profitable as well as a pleasurable point of view, is not less important to others, than their own pursuit to themselves, and that while it is for their own interest, to avoid every unnecessary damage, or the continuance of such beyond the necessity that gave rise to it, it is also well worth their while to study, and even at some cost, to circumscribe their trespasses, both in extent and duration, as much as is practicable, in order that the Occupiers

and

and Owner of the Lands may be kept in good humour, and be disposed to assent readily to every necessary indulgence.

By a judicious forecast, and contrivance of the underground works, the Air-pits or additional drawing Pits, may often be made to fall in the angles or by the sides of Fields instead of the middle thereof, as too commonly happens. Where it may be requisite to open an air-pit in a Gentleman's Park or near his House, the rubbish as fast as drawn, might be moved to some near Pit or broken ground, or to a low place, previously bared of its top soil, to be re-spread on the rubbish as soon as levelled; and a ring of plantation made with care, might almost immediately or very soon protect and conceal the mouth of this Pit from the cattle and the view of the House and grounds, unless a tall chimney for draught, might prove necessary, (after trying to do without), in which case, any architect or ornamental gardener of good taste, might easily design such an erection, as though not costly, might prove ornamental rather than otherwise.

In ordinary cases, coals sufficient for supplying the *Fire-pan*, occasionally necessary in the Air-pit, might be reserved near its bottom, and worked and drawn here by two Men, who need require only a single path, to approach and enter the small ring of plantation appropriated to their operations: and in case of safety Ladders being provided in such a Pit, the ingress of the Colliers to the reserved grounds, by this means, might be prevented, by a tall fence and locked Gate within this ring of plantation.

In case of old Coal Hollows standing full of water at the time of putting down *a new foundation*, that is, sinking an Engine-Pit *more in the deep*, on the same Coal, it is rarely advisable to ever let this water down to the new Engine, but a separate Engine should be erected on the old level, to continue to lift this water to its outlet: and in case of the proper place for such an upper Engine, being now occupied by a Park or the near vicinity of a House, &c. it frequently would be practicable, to choose a situation not far distant, in some gully by a Road, or behind a Hill or tall Grove, where a new Engine-Pit might be sunk, and a level from its bottom be driven, to meet the old Coal-level at its nearest point, and thus, without material annoyance to any one, especially if the Engine fire burn its own Smoke, the old accumulations of Water might be gradually all raised, without unnecessary magnitude of Engines and Pumps, long before the new Works could approach the old ones, so as possibly to incur similar danger, to that of late years pending at Heaton, and as is still doing at many scores of Collieries in Great Britain, particularly where

where the old Basset Hollows, or drowned *rise-works,* are in different Estates from those in which the new foundations have been put down.

It has often appeared to me to be improper, that a Coal-owner *having wrought out his Coals* to his very boundary line in the deep, and perhaps beyond it in places, as too commonly happens, by accident, and left an immense *reservoir of water underground, which did not originally exist there, resting against the Coals of his neighbour* in the deep, should be able to insist on retaining this *in terrorem* over such neighbour, for suddenly overpowering his Engines, unless the same are made much larger than necessary, and perhaps of drowning a great part of the Men in such deep works!.

A general Law, appointing *Commissioners of Mineral drainage and ventilation,* on similar principles to those so very long and beneficially acted on by the *Commissioners of Sewers,* on or near the surface, might remedy these hardships, in the ways I have suggested above, or any other more equitable and eligible modes, which the professional abilities that they would be enabled to call around them might devise and recommend: such Law, to empower Air-pits to be opened and maintained (at the expense of the party wanting them) for the purpose of freeing the dry old Coal-hollows in Estates under different owners or tenures, of their not less fatal reservoirs of noxious Airs, *accumulated in modern times by the acts of the parties,* and therefore fit subjects for legal removal, in common with *recent nuisances of every kind,* for which our Laws provide the remedy.

Your Correspondent N, in page 365, very properly hints, that the *drifting* through the *up-cast Dike* or Fault, in the efforts making in *the rise* of Heaton Colliery, for letting off the water of the northern division of Heaton-Burn Colliery, ought to have induced more subsequent *caution:*—indeed, the cutting through this natural barrier, ought not to have been attempted (and perhaps was not) without first *boreing* through the Fault-stuff and several yards beyond, in the obliquely rising direction in which the Coal-hollows lay on the other side; and this *preceding of the drift, by a bore-hole of several yards in length,* ought not to have been omitted on any account, until the water was thus first tapped by the bore-hole, and through which it might safely discharge a part at least of the penned waters: it unfortunately, however, appears, that the Overseer in this case, from insensibility of the danger he was running, and inducing to others, neglected this precautionary boring, when most wanted; and even when the Drifters pointed out the alarming dripping of water from the joints in the bottom of the Coal-seam, which then formed some space of the roof of the oblique ascending drift, instead of immediately ordering

dering the Borers, and sending all the Men out of the Pit, except two or three necessary for the boring, or at least apprising the whole of their imminent danger, and stationing persons to give instant alarm for their escape: the operation of boring was not only put off several hours, but directions seem to have been given for increasing the danger, in an eminent degree, in the mean time, by "squaring up the work," that is, working out the angles of the sides and end of the drift, ready for measuring; by the commencement of which operation, it seems probable, that the under surface of Coal in the roof of the drift, so fatally pressed by water on its upper surface, was enlarged, and it was enabled at once to fall, to the large extent which must have happened, to so quickly fill with water the large empty spaces in the lowest parts of Heaton Coal-works.

Some persons, from not duly considering the distinctions that exist, between the cases of the Boatswain or other petty Officer commanding a boat's crew in cutting out an enemy's ship, in their perfect (and truly lamentable) contempt of danger, to their own lives and those of great numbers of others, may think it improper, that even an oblique censure should be thrown on the habitual hardihood, or even the temerity of Coal-overseers and their Men; or at least, that a veil ought studiously to be thrown over the errors of those, who have fatally suffered for the same. I cannot however subscribe to such a doctrine, or think it other than the duty of those who may happen to be able, to give to the public explicit and full information on the circumstances attending events, which cannot fail of exciting their interest and sympathy, in order that past errors and dangers needlessly hazarded, may operate to the prevention of similar or analogous ones in future: every further communication therefore, of your Readers on the spot, who can throw new or further light on the case of Heaton Colliery, ought to be, and I doubt not will prove acceptable in your pages.

It will be recollected by many of your Readers, that the doom of the many unfortunate Colliers who perished in *Felling* Colliery SE of Newcastle, in 1812*, was supposed by many on the spot, to be sealed, by the necessity which existed, of closely covering over all the pits of that Colliery, soon after the Fire-damp explosion happened, in consequence of the same having *set fire to the loose Coals*, in some parts of the Works, and which fire there seemed no other immediate mode of extinguishing. This is not a very uncommon calamity, following the gaseous explosions in Coal Works. On inquiry in the proper quarter, I have been informed, that the burning of some refuse Coal, at the Coal-

* See the Monthly Magazine, vol. xxxv. p. 649.

pit

pit at *Brora* in Sutherland, in the North of Scotland, which happened last Summer, of which mention is made in an anonymous paragraph in p. 314, of your April number, did not happen through any peculiar property of these Coals, occasioning their spontaneous combustion, as is there asserted, but happened, not on the *pit-hill*, as any one reading this loose and extraordinary notice might have supposed, but below, in the works, and is said by my Correspondent, to have been solely occasioned by the neglect of preserving proper air-gates therein, as I will mention below; by which neglect, inflammable gas was accumulated, although the same is evolved in very small quantities only, in these works, and the accidental firing of this gas, set the *gob* or waste Coals and rubbish on fire, that had been improperly left in loose heaps in the works; but which was very soon extinguished, and the works soon after resumed, instead of the Pit remaining shut up six or seven months after the event, "partly on account of this peculiar property *of the Coal*," *i. e.* of spontaneous deflagration, as is there asserted.

In your xxxixth volume, p. 337, an account is inserted, of the boreing which preceded the sinking of the present Pits at Brora; and in vol. xlii. page 53, it is mentioned, that the workings of Coals at Inver-Brora (or Mouth of the Brora River) commenced in 1598; it seems rather surprising therefore, to see the contrary *opinion* to there being *any Coals north of the Tay*, again revived in your work, and represented as not proved to be unfounded, until *the discovery* about two or three years ago!.

As this is the most northern Coal-field known in Britain, and is in the near vicinity of mountains containing Granite, I have thought that the following account of it may not be unacceptable to your Readers. Accounts are preserved in Sir Robert Gordon's history, which I saw at Dunrobin, in 1812, that Coals were first wrought on the shore S of the mouth of the Brora River, by Jane Countess of Sutherland, in the reign of Queen Elizabeth, in 1598, and where she erected Salt-works*; also that in 1614,

* It furnishes a curious proof of *the progressive rise of the Sea*, of which I have had similar proofs on every coast of Britain, that the remaining walls of this old Salt-house are washed now to a considerable height by the ordinary Tides, which mostly flow higher than the tops of *the fire-places*, which are still visible, on which the salt-pans stood! and the tops of the Coal-pit hillocks that were made at this period, are most of them since covered by the sea-beach. On the shore at Mostyn, in Flintshire in North Wales, the Pits sunk about the year 1640, in which the fire-damp explosions happened which are recorded in the Philosophical Transactions, No. 136, and where the water-wheel and chain pumps were used, that were drawn in 1684, and have been since engraved in Mr. Pennant's "Account of Holywell and Whitford," have now long had their tops covered, by almost every Tide!.

John

John the 5th Earl of Sutherland, the son of the above Countess, re-opened these Pits.

In the early parts of the last century the Earls of Sutherland prosecuted the Inver-Brora Coal-works; and tradition points out one of the Pits of this period, in Shean Park, in which 15 Men lost their lives at the same time, by the falling in of the roof of the Pit.

About the year 1764, the working of the Inver-Brora Coal was again resumed, by Mr. *John Williams*, the since well-known author of "The Mineral Kingdom," under a lease from the late Earl of Sutherland, and at the same time, Messrs. Robertson and Mackenzie of Portsoy, erected new Salt-works there, under a lease from the Earl, and Mr. Williams contracted to supply them with Coals, at a stipulated price per ton. The Coal-seam then in work, was three feet eight inches thick, in two beds of a good quality of Coal, but having between them, an eight-inch black pyritic dirt bed; it appears, however, from the information of Major Hugh Houston, of Clyne House, who when young, assisted Mr. Williams, and has preserved many papers and documents to which he kindly allowed me access, that Mr. W's practical knowledge of collierying was then very scanty, (although in 20 years afterwards he acquired so much knowledge and reputation in this art,) so much so, as not to discover the mischief, of cutting down this pyritic dirt among the Coals, which also the smallness of his Coal-rooms and mode of working, rendered exceedingly broken and small: nor did he discover (as will appear from his Min. King. 2d Edit. vol. ii. p. 32) that *this dirt* among the broken Coals, *occasioned the spontaneous firing*, of a large heap of these small mixed Coals on the Pit hill* at *Inver-Brora*, or the firing of a Cargo of them at Sea, in a vessel which was conveying them to Portsoy.

These defects of management, occasioned Mr. Williams's sale of coals to fall off, except to the Salt Company (whose pans and grate-bars were rapidly wasted by the use of these foul Coals), and his affairs to become embarrassed, and being also threatened by the

* It seems to have been this circumstance, of near 50 years standing, which has been revived, mixed up and confounded with other recent events *at Brora Coal Pit*, half a mile distant from this spot, and on a quite different seam of Coal, by the writer of the paragraph in page 314: who errs also, in supposing it, *to be peculiar to the Brora Coal*, or rather to its accompanying dirt bed, to fire spontaneously. Since Mr. Williams mentions another instance at Ayr in Scotland; and at Heanor, Ripley, Denby-Hall, Donisthorpe, and other Collieries in Derbyshire, a thin dirt bed, swells and heats on access of the air, and actually fires the loose waste Coals, if mixed with them, as I have mentioned in my Report on that county, vol. i. p. 348; wherein I have also mentioned the probability, that the serious evil so well known in the vicinity of Dudley in Staffordshire, of the waste small Coals in

the Kirk officers, on account of a natural child which was born to him (who was still living near Brora in 1812), he gave up his Coal-Lease and Works, to the Salt-Company, and in the year 1769 removed to East Lothian, where the foundation of his well-deserved fame as a writer on Coal Works was laid, and his work compiled.

Mr. Houston succeeded to the management of Inver-Brora Coal-works, for the Salt-Company, merely pursuing the system he had seen under Mr. Williams, until January 1776, when Mr. William Beaumont, a coal-viewer from Limekilns in Fifeshire, being employed to examine and report on these Coal-works, he first pointed out the defects of management that have been mentioned above:—in consequence of which, larger *rooms* were adopted in working the coal, and a very careful separation of the pyritic dirt-bed was made, as Mr. Beaumont had recommended; and thereupon, the Coals proved free from sulphur in the burning, or of any other defect, as the very ready sale of several cargoes of them at Inverness and Aberdeen, when subsequently sent there by Mr. Houston (as the produce of *a new seam*) fully proved; and which account of the quality of this seam was confirmed to me by the Colliers at work at Brora in 1812, who in the previous year had opened the Inver-Brora Coal-seam on the Shore, and raised and burnt this Coal for some time in their houses, and which, when divested of the middle dirt, proved sweet-burning, and of good quality.

The Salt-Company would now have put down a steam-engine, and entered on a spirited working of this Coal-seam, of which a considerable space remains yet unwrought; but their Lease being too near expiring, and the Tutors of the present Countess of Sutherland being unable in her minority to grant a new one of sufficient length, they soon after relinquished the concern altogether, and the Colliery at Inver-Brora has since lain unwrought.

When the Marquis of Stafford and the Countess of Sutherland his Lady, entered on their spirited and general system of improvements on this fine, but hitherto much-neglected County,

in the hollows of the thick Coal, taking fire after several months, if the external air be not sooner excluded, is owing to some *distinct bed of dirt* between the Coals, that might be found and separated, and remove this evil, that occasions the waste of so many Coals. At Lasalla, Fontaines, and other places in the Aubin Coal-field in the department of Aveyron in France, the same thing happens, see Nicholson's Journal, vol. xxix. p. 352. On Cefn-mawr Colliery Pit-hill in Ruabon, in Denbighshire, a large heap of mixed dirty Coals, intended for lime-burning, took fire, after the rain of a thunder-storm in hot weather, in 1809; and other instances might be quoted, but none I think that would show, that *Coals* themselves, in any instance take fire spontaneously.

Mr. William Hughes, a coal-viewer from Flintshire, fixed on a spot, higher up the River, where advantage might be taken of its fall, to turn wheels for pumping and drawing the Coals, if found; and which on boreing there, were found, in a new double seam, of excellent quality at 79 yards deep, the upper bed being $3\frac{1}{4}$ feet, and the lower $1\frac{1}{3}$ foot thick, separated by 2 feet of black clunch, and dipping 1 in $4\frac{1}{3}$ to the SE; several tons of which Coals had been raised previous to my examining Brora, and were daily burned at Dunrobin Castle, while I was there, and gave great satisfaction to every one, from their quality in burning; but I was deprived of the advantage of inspecting the seam myself, by the Pits then standing full of water, until the water-wheels and pumps should be finished. Although the vast beds of Gravel and blocks of stone scattered on this coast, and peat-lakes on these, rendered the Field very difficult of investigation, yet it appeared from my survey, pretty clearly, I think, that there is another workable seam of Coals, between that so long worked at Inver-Brora, and this Brora seam, and several thin ones below this, furnishing altogether a body of Coal, for ages of pretty extensive workings, or for centuries of supply to this County and its vicinity.

From the sea-shore at Dunrobin Pier, in front of the Castle, to Golspie Bridge, and thence to Rhives Farm, there appears a very singular conglomerate Limestone Rock, having in some places large masses of compact steatite imbedded in it, and in others, imbedding grains of quartz, almost exactly like the mortar of an old wall; on revising now my notes on this Rock, and comparing them with my subsequent observations on the Alberbury conglomerate Limestone (which also contains quartz grains) I see so many points of resemblance, as makes me think it probable, that both may alike belong to *the unconformable yellow Limestone*, see page 168, of your present volume, and that these very distant Coal-fields of Sutherland and Shropshire, may hereafter prove to be of the same strata?; and even, that the micaceous red sandstone, imbedding masses of varied conglomerate, in which the pieces of reddish Granite are *of all sizes*, in Sutherland, may prove to be *over-lieing and unconformable to this Coal series*, and answer to the upper Red Marl, in which the Charnwood-Forest and Malvern-chase Granites, and perhaps those of Devon and Cornwall counties also, are imbedded?.

But I must hasten to mention a few other particulars of the *Brora* Coal-work, &c. which have been communicated to me by Letters, since I returned from Sutherland. It does not appear that Mr. Hughes, who has been mentioned, was again consulted, after the Brora Pits had been sunk, by an Overseer and Men from Denbighshire, whom he brought there: but the entire manage-

management of the concern was entrusted to them, until the spring of 1814, when it being seen, that they were unequal to the task, they were changed for another Overseer and Men, engaged in the Coal-field of the Forth and Clyde: it was however soon found, that these Men managed, with even still less skill or propriety than their predecessors, "the levels had been lost, no air-roads cut, the face of the Coal irregularly carried forwards, and the gob thrown against it, and part of it permitted to get on fire," &c.

Whereupon the Marquis sent down Mr. *John German*, a Coal-overseer or Bailiff, from his Staffordshire Collieries, and a gang of Men who had been used to work there under him, who are now settled at Brora, and are successfully prosecuting the works: the level Gates have been cleared and perfected for about 100 yards SW and NE, and rail-ways laid in them: air-gates have been driven about the same distance up the rise, and two new Pits are now sinking; the *roof* has proved very sound and good, and the Coal easily parts from it: no Faults of the least consequence have been met with, only a few trifling ones or *slips*, which derange the Coal but a few inches.

From this Colliery, a Rail-way has been laid by the side of the River Brora, to the shipping-place at its mouth, and ere this, the shipment to Portsoy, Inverness, Aberdeen, &c. and to nearer places on the Coast, has I believe commenced.

Two Salt-pans had been some time erected, for evaporating the sea-water, and two others were erecting, in consequence of the success that promised to attend this manufacture. A Staffordshire brick and tile-maker had settled at Brora, and was successfully employed: and plenty of clay, found adapted to the making of stone-ware, on trial by Mr. Spode, of Stoke, in the Staffordshire Potteries, being discovered, trials were making, to get some Scotch Potter to settle at Brora. In addition to the many pleasing instances of important rural Improvements lately introduced in this distant county, which retaining still all the abuses arising out of its former feudal state, required new settling, as it were, when the Marquis and Lady Stafford and Earl Gower their Son, commenced the patriotic exertions, which are detailed in the Appendix to Captain Henderson's Agricultural Report, and from the inspection of which Improvements, of almost every kind known in England, I derived so high a satisfaction while in Sutherland; I have learnt, that a Company had engaged with the Marquis, for erecting a Tan-work on a considerable scale at Golspie, with an extensive Piggery, and houses for slaughtering and curing Pork, and perhaps Beef, for exportation to the ports of Inverness, Aberdeen, &c. as one of the means become neces-

 sary,

sary, for vending the surplus of corn and improved live stock, which this south-eastern coast of the county began to yield, instead of having nothing to spare but a few half-starved cattle, bred in the mountains in the interior.

I am, sir,

Your obedient servant,

12, Upper Crown Street, Westminster, June 13, 1815. JOHN FAREY, Sen.

LXXVII. *Queries and Observations relating to the Formation of the Superficial Part of the Globe. By* ROBERT BAKEWELL*, *Esq.*

In the present chapter I propose to offer some remarks and queries respecting the formation of the rocks and strata that compose the superficial covering of the globe, and to state the inferences which appear to me deducible from the contemplation of existing phænomena. I beg, however, to be distinctly understood as offering these observations to the consideration of geologists, without any desire to obtain their assent, further than may be warranted by the evidence of facts, or by rational probability. Whatever may be thought of the queries here proposed, they cannot, I trust, prejudice any candid mind against the preceding parts of the volume. If philosophers, instead of fabricating hypotheses, had proposed their speculations in the form of queries, in imitation of Newton (in his Optics, book 3), they would have rendered a more essential service to science. Thus relieved from the labour of defending their own systems, they would have been more free to follow truth wherever the light of evidence and induction might lead them. Many important discoveries might have been anticipated, which could not be brought forward as parts of a system, because the connecting links in the chain of discovery were still wanting. Such was the anticipation of the inflammable nature of the diamond, by Newton. When any science is just advancing beyond its infant state, the mode of proposing our opinions, in the form of queries, is most desirable; and I trust this will be a sufficient apology for adopting it in the following pages.

The geologist endeavours to make himself acquainted with the various beds and strata that form the crust of the globe; and, if possible, to discover by what process they were formed, as well as to trace the changes they have subsequently undergone.

* From the concluding chapter of his second edition of the Introduction to Geology, just published.

The

The power of man to penetrate the earth is very limited: few mines have been sunk to so great a depth as five hundred yards; yet by the fractures and dislocations of the strata we are frequently enabled to measure their thickness as they rise in succession to the surface over a considerable extent of country. But the regularity of rise is limited, and the strata are lost as we proceed further or incline in an opposite direction, so that the greatest aggregate thickness that has been any where examined does not perhaps exceed eight miles. The diameter of the earth is nearly eight thousand miles, to which the depth of surface that we are acquainted with bears no greater proportion than the thickness of a wafer to the diameter of a three-feet artificial globe.

Were we to bear these facts in mind, the fractures, dislocations, and overturnings of rocks and strata, and even the up-heaving of the bed of the ocean, phænomena which appear so overwhelmning to the imagination, sink into comparative insignificance: instead of being astonished at these changes, we should be more disposed to admire the stability of nature, in preserving the incrustation of the globe so perfect amidst the conflict of tumultuous elements. There are a few leading facts in geology, which we may consider as clearly ascertained by existing phænomena. Among these we may enumerate, 1st, That the present continents were once covered by water. 2nd, That the strata in which organic remains occur, were formed in succession over each other. 3d, That every regular stratum was once the uppermost part of the globe. Let us further inquire, whether there remain any appearances in nature that may indicate in what manner these strata were formed, or the source from whence the matter of which they are composed was evolved. The two great agents in the decomposition and formation of mineral substances, either artificially or in nature, are water and fire: any theory which should exclude the agency of either from the formation of the crust of the globe would be manifestly defective.

The numerous volcanoes scattered over the globe abundantly prove the existence of fire in the deep recesses of the earth. During a former state of our planet, this internal fire must have been more intense than since the records of authentic history. This is shown by the remains of mighty volcanic craters, which far exceed any that are active at the present time; for, the craters themselves being formed by the eruption of volcanic matter, their size bears evidence to the magnitude of their former operations.

It is natural to inquire what part these tremendous agents have performed in the œconomy of nature: Are they accidental appendages, or essential parts of the terrestrial system? The

 geologists

geologists who exclude the agency of fire from the formation of rocks, seem to forget that the only instances we have of actual rock formations are volcanic: beds and strata more than thirty miles in length, and of considerable breadth and thickness, have been spread over the surface of the globe in our own times: and according to Mr. Humboldt, the further back we trace these eruptions, the greater is the similarity between the currents of lava and those rocks which are considered by geologists as the most ancient. The enormous volcanoes whose craters are many leagues in extent had doubtless an important office to perform in nature: and can it be unreasonable to believe that the earth itself is the great laboratory and storehouse where the materials that form its surface were prepared, and from whence they were thrown out upon the surface in an igneous, aqueous, or gaseous state, either as melted lava, or in aqueous solution, or in mechanical admixture with water in the form of mud, or in the comminuted state of powder or sand? Inflammable and more volatile substances may have been emitted in a gaseous state, and become concrete on the surface.

These primæval eruptions, judging from the size of the ancient craters, may have been sufficient to cover a large portion of the globe. Nor can it be deemed improbable that still larger and more ancient craters have been entirely covered by succeeding eruptions. In proportion as the formation of the surface advanced, these eruptions might decline, and, when their office was performed, might finally cease.

It is not necessary to suppose that these subterranean eruptions consisted only of lava in a state of fusion. The largest active volcanoes at present existing, throw out the different earths intermixed with water in the form of mud. Nor should we limit the eruptions of earthy matter in solution or suspension to the known volcanic craters: the vast fissures or rents which intersect the different rocks may have served for the passage of the subterranean matter rising to the surface. Silex or quartz, either pure or combined with other earths, constitutes two-thirds of the crust of the globe; and the veins which intersect the lowest granitic and schistose rocks are most frequently filled with this mineral. Whether the elementary parts of silex are easily soluble in water, or fusible by fire, we have yet to learn; but we know there are natural processes by which its solution is effected, and may not the strata of crystalline sandstone have been formed from these solutions? Calcareous or cretaceous matter is also ejected during aqueous eruptions. (See page 317.) The beds of limestone may have been formed by similar calcareous eruptions; and the numerous remains of shell-fish in limestone might appear to indicate that the calcareous solutions were favourable

to

to the growth of animals whose coverings contain so much calcareous matter. Nor is it necessary to suppose that these aqueous eruptions were always sudden, and attended with violent convulsions; for, when a passage was once opened, they may have risen slowly and been diffused in a tranquil state, and by gradual condensation may have enveloped the most delicate animals or vegetables without injuring their external form.

The long intervals of repose between the great igneous volcanic eruptions may have allowed time for the growth and decay of animals whose remains are found in different strata; whilst the formation of other strata may have taken place, under circumstances incompatible with organic existence: and accordingly we find in the rocks most abounding with organic remains, certain strata in which they never or rarely occur. The same agent which enveloped living animals in mineral matter without injuring their external form, appears in some instances to have immediately arrested the functions of vitality. Petrified fish have been discovered in solid rocks in the very attitude of seizing and swallowing their prey. A sudden eruption of a hot fluid saturated with the different earths (or the elements of which these earths are formed) might destroy in a moment the animals previously existing, and form round them a siliceous or calcareous incrustation which would protect their remains from further destruction.

Ages of tranquillity might elapse in the interval of different eruptions, and beds of gravel and breccia be formed by the gradual disintegration of the higher parts of the earth. These beds might be afterwards covered by, or intermixt with, the crystalline beds from subsequent eruptions; and may we not in this manner explain the alternation or intermixture of crystalline rocks with those of mechanical formation? Dislocations of the strata by earthquakes and other causes might also take place between the periods of different formations, in which case the upper beds would rest on the subjacent ones in an unconformable position.

As the strata which cover each other are often composed of very different mineral substances, may we not infer that the successive ancient eruptions, whether igneous or aqueous, contained different elementary parts? At the present day, the lavas of succeeding eruptions even from the same crater differ both in external character and constituent parts. Hence we may explain the formation of strata of ironstone and beds of other metallic ores alternating with earthy strata; and we can have little difficulty in the admission, as it is now known that the bases of the earthy strata are also metallic. Two or more mineral substances may in some instances have been contained in the same fluid, and separated into different masses or strata by the laws

 of

of chemical affinity: but it seems exceedingly difficult to admit, with the Neptunian geologists, that all the substances which compose rocks and strata were coexistent in the same fluid, and that this fluid after it had deposited only a small part of its contents was capable of supporting animal life.

The succession of aqueous and igneous eruptions would account for the alternation of volcanic rocks with others of aquatic formation. The occurrence of obsidian and basalt with clay and sandstone may be parts of the same series of phænomena; and thus the two opposing systems of Werner and Hutton may both be true to a certain extent, and agree with existing facts. However vast these operations may appear, they sink into insignificance, compared with the bulk of our planet itself. If a three-feet globe were to contain within it a fluid capable of acquiring consistence by exposure to the air, and were this fluid from time to time to exude through minute cracks or punctures, and form over different parts of the surface successive coats of varnish whose aggregate thickness was less than that of a wafer, this would be a greater change with respect to the artificial globe, than the formation of all the rocks and strata with respect to the earth. And the numerous dislocations and fractures, by subsidence or other causes, are no more in comparison to the magnitude of the earth, than the cracks or inequalities of this superficial varnish would be to a globe of that diameter.

I have already stated my opinion *, that all the secondary strata are local formations originally deposited in detached lakes, which have covered part of our present continents when the sea began to retire; for the inequalities of the surface must have been greater before the deposition of the upper strata had filled up the lower concavities. In proportion to the quantity of matter thrown from the interior of the earth might be the subsidence of the surface in other parts; and as the waters retired further from our present continents, the size of the lakes which then covered them would be diminished; but their number would be increased, and also the number of local, or independent, formations of strata. Similar causes still continuing to operate in different situations, might produce general features of agreement amidst the diversity of rock formations which were taking place. Now this is precisely what we observe in comparing the succession of rocks in distant countries. We have no sufficient reason to believe that those rocks which are called primitive, are in reality the original coat of the nucleus of our planet, nor that the similar rocks of distant regions are contemporaneous; the great diversity which prevails both in their order of succession and composition appears to oppose the theory of universal formations,

* Chapter X.

inations, there being no two countries in which the order of succession is found to agree; and a recent examination, by Raumer, of the very district in which the Professor of Freyburg laid down the law of succession for the whole globe, is said to have shown that Werner's descriptions do not even agree with the actual order of succession in which the rocks of that district are arranged.

Granite, porphyry, sienite, green-stone and basalt pass by such insensible gradations into each other, and into rocks known to be volcanic, that the probability of their having a similar origin can scarcely be denied. And if the internal fires that have acted successively on the surface of the globe were of vast extent, as the remaining craters indicate, they may also in numerous instances have melted or softened pre-existing rocks and strata, and occasioned the bending and contortions of the strata, and other phænomena on which the theory of Dr. Hutton was founded. The defect of that theory consists, I conceive, in extending the operation of this cause further than existing appearances will support.

Were we to admit that rocks are local formations produced by successive igneous and aqueous eruptions forced through craters and fissures of the surface, these, with subsequent elevations and subsidences of the surface, might be sufficient to explain all the various phænomena which the position, contortion, succession, and alternation of rocks and strata present to our notice. In some situations granite mountains are covered with a series of schistose rocks, to which succeeds the mountain limestone, and on this are laid the sandstones of the coal formation. In other instances these sandstones rest immediately on granite, without the intervention of schistose rocks. Here then we may suppose that no eruptions of matter took place between the formation of the granite and the sandstone; while in other situations a succession of formations had produced all the intermediate rocks. In some countries the eruption of matter which formed granite, after ceasing for ages had again taken place, and thus sometimes we find granite covering rocks to which it is most frequently subjacent. To a like cause may we ascribe the occasional appearance of beds similar to the lower rocks alternating with or appearing in the upper strata. The siliceous and calcareous solutions in a state of tranquillity might also envelop the fragments and sand from pre-existing rocks, and form the various breccias and aggregated sandstones. Saline and bituminous matter may also have been thrown up in detached lakes, and subsequently consolidated, as in the pitch-lake in the Island of Trinidad. The local formation of beds of trap alternating with other rocks has before been alluded to, and the graduation

of

of basalt into clay, or sand, will be consistent with this mode of formation. Many of the solutions containing terrene matter might be erupted at a boiling temperature, like the siliceous water thrown out of the hot springs in Iceland, and on cooling they might deposit their contents, the matter from each eruption forming a separate layer or stratum *.

In some parts of the earth the quantity of matter thrown out during one eruption may have been sufficiently great to admit the crystallization of whole groups of mountains. In other instances it may have been so widely diffused as to form very thin strata. And here it may be proper to remark, that different beds and strata are not arranged in nature in the order of their specific gravity; the lowest are not always the heaviest, neither are they arranged according to their more perfect crystallization; for, though generally the lower rocks are more crystalline than the upper, we not unfrequently find some of the upper strata more perfectly crystalline than the subjacent rocks. Now if the matter of which the upper and lower rocks are formed had been co-existent in the same fluid medium, one or other of the above effects must have taken place; but if each stratum were formed by a separate eruption and deposition, they might vary both in specific gravity and degrees of crystallization, without any regard to the order in which they were deposited †.

In endeavouring to trace the causes of very complicated phænomena, those explanations are to be preferred which apply to the greatest number of cases, and are consonant with existing or analogous facts. Now I conceive that the alternation of aqueous and igneous eruptions offers a more satisfactory explanation of the formation of rocks than any that I am acquainted with. At the same time it assigns an office to the immense craters and fractures which have once perforated or intersected the globe.

It is an acknowledged maxim, that Nature, or to speak more

* To compare great things with small, there is an analogous formation taking place every day in the channels which receive the boiling waters from some of the steam-engines in the county of Durham. This water contains a large quantity of earthy matter which is deposited every day, except Sunday, in regular layers that may be distinctly counted, with a marked line for the interval of repose on Sunday, between each week's formation: hence the stone got out of these channels has received from the country people the name of *Sunday stone*.

† By considering each stratum as a local formation, we are relieved from the difficulty of accounting for the disappearance of the vast beds of sandstone and chalk, with all the upper strata, in countries where they are not found at present. Could we be presented with an accurate delineation of the elevations and depressions of the earth's surface, sufficient vestiges of its ancient physical geography might still remain to enable us to trace some of the great basins, or lakes, in which the separate formations of the upper strata took place.

correctly

correctly its divine Author, does nothing in vain; and can we suppose that the interior part of the earth is constructed with less skill than what we observe in the organization of the simplest animal or vegetable? Or, when we contemplate our planet pursuing its trackless path through the heavens with unerring precision, can we believe that its internal motions are not governed by determined laws destined to answer the most important purposes in the œconomy of nature?

Though I am inclined to regard the explanation here offered respecting rock formations as consonant with existing facts, and as reconciling the phænomena of aqueous and igneous products alternating with or graduating into each other,—facts that appear so contradictory to the theories hitherto advanced,—I would, however, willingly adopt any other explanation that may afford a more satisfactory solution.—The Roman poet, after conducting his hero through the subterranean abodes, dismisses him through the Ivory Gate*: and should my readers infer from these speculations respecting the subterranean operations of nature, that I take my leave of them in the same manner, it will neither cause disappointment nor excite displeasure. Embarked with them in a voyage of discovery, I shall gladly hail the signal for the appearance of solid ground, whoever the fortunate discoverer may be.

LXXVIII. *Accident at Newbottle Colliery on the Wear. By* A Correspondent.

To Mr. Tilloch.

Sir,—I feel myself much obliged to you for giving publicity in the last number of your Magazine to the account I transmitted of the dreadful accident which happened at Heaton Colliery, on the 2d of May: for it is to be hoped that the coal-owners of this district, when they see these melancholy catastrophes communicated to the world at large on every recurrence, will at length be impelled from a sense of shame, if humanity has no weight with them, to take some effectual step, under the sanction of an act of parliament, towards providing a permanent and sufficient fund for the support of the numerous widows and orphans of the miners whose lives are sacrificed in their employment. From a motive of compassion, therefore, I now resume my pen to detail another of these shocking occurrences, which has proved nearly as destructive of human beings as that at Heaton, though in this instance fire, not water, has been the agent of death. Newbottle

* Virg. Æn. lib. vi.

colliery

colliery, the scene of the disaster, is situated on the river Wear. At present the proprietors are working the Hutton main; the deepest and best of five beds of coal within the royalty, its thickness being six feet two inches, and, like most seams subjected to carburetted hydrogen, nearly destitute of water. This mine was won about four years ago, and is carried on by the means of three shafts; one, called the Success Pit, is one hundred and eight fathoms deep. At five o'clock in the afternoon of the 2d of the month, a cloud of dust and smoke was seen to issue from the mouth of this shaft, by which the workmen at bank were convinced that an explosion had taken place below ground, and in a few minutes one of the trappers, who was not above six years of age, cried out to be drawn up; he was quickly followed by fourteen men and boys, most of whom were shockingly scorched, four only having escaped the effect of the inflammable gas. But a short time was allowed to elapse before several intrepid pitmen descended into the mine, where they found the corpses of fifty-seven of their unfortunate fellow workmen stretched on the floor;—some of whom appeared to be burned to death, but the greater number to have been suffocated by the after-damp, or azotic gas left by the combustion of the hydrogen with the oxygen gas. Some few still retained signs of life, but expired on being brought into the atmospheric air. From these circumstances it is evident the blast was partial; for many of the men had quitted the boards where they had been at work apparently unhurt, but met their fate on the waggon-way, being suffocated before they could reach the shaft. Of the nineteen horses in the mine, six only were killed; those in the stables having survived, for the air-courses were soon restored. It is asserted that the inflammable air which occasioned the disaster, issued from an adjoining waste carelessly holled into in the course of working, but I believe this point has not as yet been ascertained.

Heaton Colliery is still inundated, and the water pumped from it has become highly offensive to the neighbourhood from the putrescence of the animal matter it contains.

Newcastle-upon-Tyne, June 10, 1815. N.

P.S.—When speaking of the depth of the High Main at Heaton, I should have said "where there was 25 fathoms *less* covering on the seam," instead of "25 fathoms covering on the seam."

LXXIX. *On*

LXXIX. *On a Contrivance to help defective Vision.* By JOSEPH SKINNER, *Esq.*

To Mr. Tilloch.

SIR,—PERMIT me to enter into a brief detail of a contrivance I fell on some years ago to help defective vision. I reached Malta in the summer of 1808, and was not long in experiencing the rapid decay of an organ already impaired, through the effect of the powerful glare of light reflected there by the white surfaces which continually meet the view, the houses and inclosures being of stone, and the surface of the island in general rocky, with patches of green, chiefly seen from the eminences, interspersed at intervals to cheer the sight. After a lapse of two years my vision became so obscured, that I could not recognise an individual acquaintance at a short distance; but by looking through a fine aperture made in a very thin metallic substance, and held close to the eye, I found the surrounding objects so completely defined, that I could see them distinctly when looking towards each extremity of the street in which I dwelt. This contrivance, by the admission of a small portion only of direct rays, without the interposition of any oblique rays of light, allows the object to be seen at any degree of proximity, or of distance proportioned to its magnitude; insomuch that when an object, very fine print for instance, is brought close to the eye, it is powerfully magnified, agreeably to a well-known law of optics; and being distinctly seen at a remote distance, it follows that an indefinite focus is obtained, always relative to the size of the object viewed. Whenever I had recourse to this expedient, I constantly found, on withdrawing the instrument, that I could distinguish objects with more precision than before; and, as I conceive, for this reason, that having been looking for some time through a chastened medium of light, I was better enabled to meet the broad glare than I should have been if constantly exposed to its action. In the employment of convex spectacles, or magnifiers, the reverse happens: the rays of light being then concentrated, the surrounding objects, when they are withdrawn, become for a time less distinct than before their application. In this view the contrivance of metallic spectacles may be considered as a preserver of the sight, independently of the advantages I am about to detail. Since my return to England, Mr. Thomas Jones, optician, No. 62, Charing Cross, has with great ingenuity contrived a pair of adjusting metallic spectacles, by the means of which he measures the distance between the pupils, so as to adapt the instrument to any individual, and bring the two sights into

into one. On the principle above explained, of the admission, through the small aperture, of a portion of direct rays only, the peculiar quality of a defect of vision ceases to be of any importance, the contrivance being equally adapted to short-sighted persons having a great convexity of the eye, or to those whose natural lens has been flattened by time. The cases which have already presented themselve are in full proof of the efficacy of the metallic spectacles. In three instances of mal-conformation where concave glasses have not afforded any relief, they have enabled those on whom the trials were made to distinguish objects clearly, thus exercising an organ of the use of which they had been hitherto deprived.

Further trials will, I have no doubt, give a wider scope to their application. As they are calculated to meet any state of defective vision, where the two eyes, instead of being alike, are, as frequently occurs, of an entirely different conformation, they may, with their aid, be brought into equal exercise. In *strabismus* or squint, where the distortion of the pupil, as generally happens, lies chiefly, if not wholly, on one side, the small aperture may on that side be gradually brought in an oblique line, towards the centre, until the pupil by habit finds its true station, by its efforts to gain the light. There will be a less strain on the optic nerve, which is always affected in the case of these distortions; and it will recover its tone in proportion as the pupil takes its right position. It is needless to observe that the exercise of vision should be confined to the side on which the distortion lies, the other eye being covered by an imperforated metallic plate, so as to shut out its view. Where the distortion is on both sides, the remedy is obvious.

As a substitute for an eye-glass, a single metallic spectacle will be found of great utility, wherever the light is strong enough to admit of its use. Without-doors it can be constantly employed with the best effect; and has the advantage of durability, not being liable to any accident. It frequently happens that an eye-glass adapted to a particular defect of sight cannot be found.

The smallness of the field which presents itself in looking through the metallic spectacles is an inconvenience which will be remedied by use. They can only be employed where there is a sufficient light; but this light being chastened in a considerable degree, they become preservers wherever a powerful glare is to be encountered. Where the sight has been much impaired by time, it may be recommended not to exercise it in a broad light, unless through this chastened medium. The strain on the optic nerve will be thus avoided. In southern climes, where there is

an intensity of light, this contrivance would afford a double advantage over coloured glasses: objects would be seen of their true colour; and the excess of light, which operates so powerfully in impairing vision, effectually corrected.

I am, &c.

J. Skinner.

LXXX. *On Metallic Salts. By* A Correspondent.

To Mr. Tilloch.

Sir,—Having continually observed that metallic solutions had an excess of acid; although it appeared contrary to the opinion of those whose merited reputation in the chemical world is so firmly fixed, I was led to think that this acid was essential to the salt; and the few experiments I made on the subject seem to justify my doubts.

Sulphate (or rather the super-sulphate) of iron was the first salt I examined. It was submitted to a heat sufficient to drive off a portion of the acid (as I knew without this process the solution was acid): water being added, the super-sulphate was in solution, and some oxide precipitated. I endeavoured to saturate the excess of acid by an alkali; but before it could be neutralized a part of the oxide precipitated, leaving the super-sulphate in solution. If the acid was not a component part of the salt, it might be saturated with a base, provided the salt produced would not decompose the metallic solution:—if therefore it was not essential, the alkali would saturate it, and the whole form a neutral solution; for the dissolved salt is not subject to decomposition by treating it with an alkali saturated by the corresponding acid to that which the metal is dissolved in.

In the case of mercury, the salts termed the super-sulphate, the sulphate, and the sub-sulphate, show acid properties when dissolved:—if then the sulphate and the sub-sulphate in the attempt at solution are converted into the super-sulphate, it is natural to infer that they are not perfect salts, but peculiar mixtures of the super-sulphate with the oxide.—And as the excess of acid in the super-sulphate is held by so small an affinity, it is not more entitled to be called an acid salt than those whose bases possess a superior affinity for their acids, and are not like this decomposed by water. Why I mention the combinations of mercury with the sulphuric acid is, that in a publication produced in 1809, it is observed that, by repeatedly washing the super-sulphate with small quantities of cold water, the acid is carried off, and "a truly neutral metallic salt remains, the sulphate

phate of mercury." Now to wash soluble crystals in small quantities of water for the purpose of freeing them from any extraneous matter, is in the operations of chemistry the most imperfect; for a salt, without entire solution, cannot be subject to the influence of water on all its parts; and as the washings are acid from the commencement of the operation till after the yellow precipitate is formed, a neutral salt cannot be produced in any part of the process, but the whole of the mercurial salt is carried off in the state of a super-sulphate. If without the aid of water we cannot discover the existence of acids (which is obvious in those which are crystallized), any dry substance being treated with water, and answering the acid tests, cannot in the dry state be considered neutral; as the properties of acids depend so much on their combination with water; and as when water is added to the above-mentioned salt (the supposed sulphate) it is acid, it must in the dry state be an acid salt with an excess of oxide mixed with it, caused by the evaporation of part of the acid in the process of drying. I therefore conclude, in the opinion that metallic salts cannot exist in the neutral state; for as in the humid way we discover acid properties in this class of salts, and as acids in the dry state do not possess acid properties, we must allow the metallic in every state to be super salts.

Your obedient servant,

London, June 15, 1815. H.

P.S.—This humble attempt, sir, is founded merely on the success I had with those salts which I at the time happened to be in possession of.

LXXXI. *On Isochronous Time-Keepers. By Mr.* Thomas Reid.

To Mr. Tilloch.

Sir,—In your Philosophical Magazine for May last, a gentleman long eminent and respectable as a philosopher in the arts and sciences "has presumed" that the two clocks of mine, whose rates of going were inserted in your Magazine of last March, "might have been in the same circumstances" as those which he has mentioned. These kind of circumstances are not new to me; such or similar observations of two clocks going on in *this way*, and whose pendulums affected each other, were made long ago by the late ingenious Mr. John Ellicott, and may have been made by others besides him, for aught that I know. But these clocks were not properly fixed, being on such elastic boards as those which Mr. De Luc mentions. I should have thought

thought my time very ill applied in attempting to regulate those astronomical clocks (with which I have been engaged) to that degree of nicety to which of late years I have been accustomed, had they been on such flimsy fixtures as those in question. The two clocks, the rate of whose going and keeping so closely together I gave you, were free of every floor, or of any elastic board whatever, were strongly attached to a firm wall, and every part connected with the pendulums uncommonly well bound, particularly in that of their suspensions, so that any extraneous weight, or any motion on the floor, could not possibly affect them, nor was it possible that the motion of either pendulum could have affected that of the other. It is nevertheless wonderful how the motion of a pendulum may be affected, even where every part of the fixture of it is such, that little or no doubt can remain of its not being strongly fixed; and yet I have made their rate alter not a little by afterwards forcibly driving home for a second time the screws of the fixtures, in order to be certain that all was firm and secure, when I well knew that the first fixing was much beyond that of any pendulum which had come under my notice. The consequence which followed the second fixing here was *quite natural*, the arc of vibration being a little increased to what it was before. There is no man in the kingdom who has paid that attention to the firm fixing of a clock pendulum which I have done, and none know better the great advantages of it. The second turret clock which I made about twenty-eight years ago, had the scapement part of it in a *frame* separate from that of the clock, which together with the pendulum (whose ball a sphere about 70lbs. weight) were firmly attached to the wall of the church, and at a little distance from the clock itself*: the advantage gained by this was not obtained but at some expense. There is a mistake made in Mr. De Luc's paper regarding Harris's pendulum clock *being erected*

* This clock, after having been brought to mean time, was carefully observed from time to time by means of a sun-dial, which was correctly put up close by the Observatory at Hawk-hill and by those concerned with the Observatory, (I not having then the advantage or use of a transit instrument,) and did not deviate thirty seconds from mean time during the course of eight months. Not that I think much of such trials, or the comparing the going of a clock with a sun-dial, where nice observations are required; yet allowing the error to be even the double of what the result gave, it cannot be called great. Seeing the good going of the clock, the person who had the charge of winding it up was requested to pay particular attention to it, and by no means to neglect the winding of it. However, this request was not strictly complied with, and the clock was one day unfortunately forgot, and allowed to run down. This circumstance, and others which soon afterwards supervened, made me in future to bestow less attention to it than it otherwise merited. The pendulum-rod of this clock was *a wooden one*, 156·8 inches in length.

 in

in 1814. I think you got it from me, stated *to have been put up in the year* 1641, as is Mr. Grignion's account of it.

I am, sir,

Your most obedient servant,

Edinburgh, June 20, 1815. THOMAS REID.

LXXXII. *On the Electric Column of Mr.* DE LUC.

To Mr. Tilloch.

SIR,—SINCE you did me the favour of inserting in your valuable Magazine a description of my contrivance for applying clockwork to Mr. De Luc's electric column, for the purpose of observing with greater facility and convenience its meteorological phænomena, and for attempting to procure by its means a measurement of time*, I have had the high honour and gratification of a short correspondence with the very respectable and able inventor, on the title of that description which I so carelessly omitted to affix, and which you were so obliging as to supply. I sincerely thank you, sir, for having so done, and have no doubt that

* In your last number, Mr. Singer asserts that a "Mr. Lightfoot first suggested the employment of an inflexible pendulum as a means of converting the reciprocating motion usually produced by the column into a source of rotary movement; and the correctness of this idea was soon afterwards practically verified by me," (whom he chooses to designate by the appellation of his pupil) "with the assistance of a watch-maker."

He also observes, that "the rotary motion obtained by this *indirect* means is however rather curious than useful, for it is *scarcely* so correct an indication of the power of the column as the simple pendulum, &c." I do not notice the first remark, for the purpose of claiming either for myself or for Mr. Gorham the watch-maker any merit in so simple a contrivance, but to protest against the ambiguous construction of the paragraph, by which, if he does *not* mean it, he *appears* desirous of having it understood, that I either directly or indirectly derived the idea from the suggestion of Mr. Lightfoot, and thus appears to insinuate plagiarism. Now, even if I thought he *did* mean to make this insinuation, I would not trouble either *you* or him with its confutation.

I notice the second remark, only for the purpose of observing that his instruction would in this case lead those into error who might be induced to avail themselves of the advantages which I derive in using the clockwork, since I have found, by having for some time past, and under a great variety of circumstances, carefully compared it with *all* the other modifications he describes, that it is not *scarcely* but *perfectly* as correct an indication of the power of the column as any one of those modifications. I suspect that the "considerable irregularity" observable in the motion of a pith-ball suspended by a silk thread, is not occasioned wholly by the variation of the temperature of the surrounding medium, and the state of moisture, &c. but partly by a tendency to stick to the balls against which it strikes, and will one day cause it to stop. To avoid this inconvenience, I first attached

that in choosing the term Electrico-galvanic agency, your view of a subject (containing so many contrary opinions) is founded upon a far more extensive knowledge of it than I can pretend to. Mr. De Luc remarks that the instrument is not to be called electrico-galvanic, but electric column as he has named it, "since there are no galvanic and only electric effects." The reason of my concurrence with him in the use of the terms electric column and electric agency is, because I think his ingenious experiments and forcible deductions, and their subsequent confirmation by the experiments of Sig. Zamboni and others, go far to prove the truth of the proposition which Mr. De Luc here advances. At all events, he has started game which may afford good sport; and it no doubt will do, so if it is pursued with the indefatigable ardour which he has so eminently exhibited in his several valuable writings and successful labours.

I remain, sir,

Your much obliged and humble servant,

Hammersmith, June 6, 1815. FRANCIS RONALDS.

attached a fine platina wire as a girth round my cork-ball, and found that its indications then agreed with those of the other pendulums and needles; but have now procured from my good friend Mr. Teid a ball of silk with a very fine platina girth, so light that it weighs but little more than the cork.

I presume it has been shown, that the frequency or quantity of the electricity of the column at different intervals does not always bear a constant ratio to its intensity; and therefore, that any apparatus where a pendulum in constant *limited* oscillation is used, cannot measure both these powers. Even the frequency with which the leaves of Mr. Bennett's electrometer open and strike the sides for a certain number of seconds, does not become a *comparative measure* of the power of the column at different times, if this expression is used in a general sense.

I have a small column to which is attached a large electrometer for measuring by the divergence of the leaves its intensity, and to which I sometimes attach a smaller electrometer for measuring its frequency by the striking of the leaves. This apparatus proves the fact very clearly; for sometimes, when the large one indicates a certain intensity, the small one by being applied to the column will strike its sides much more frequently than at others, although the divergence of the large one remains the same, or has even diminished. Sometimes the intensity and frequency will go on to increase together in corresponding ratios to a certain maximum, and then alter. At other times the former will stop, and the latter go on to increase. These phænomena depend, I believe, principally upon the variation in the hygrometric state of the disks of paper. The influence of moisture to increase the general power of the column to a certain extent Mr. De Luc first observed. On columns inclosed in glass tubes it of course cannot act so powerfully; and moisture, by being deposited on the glass, may, as Mr. Houldy has shown, diminish its power by injuring the insulation. But, by subjecting a column to the action of a moist and a drier atmosphere whilst its supports were not so circumstanced, I think I have proved that, in the usual hygrometric state of the air, the state of insulation has little or no share in the above phænomena.

LXXXIII. *No-*

LXXXIII. *Notices respecting New Books.*

Philosophical Transactions, Part I. for 1815, has made its appearance, and the following are its contents:

I. Additional Observations on the optical Properties and Structure of heated Glass and unannealed Glass Drops. By David Brewster, LL.D. F.R.S. Edin. and F.S.A. Edin. In a Letter addressed to the Right Hon. Sir Joseph Banks, Bart. K.B. P.R.S. —II. Description of a new Instrument for performing mechanically the Involution and Evolution of Numbers. By Peter M. Roget, M.D. Communicated by William Hyde Wollaston, M.D. Sec. R.S.—III. Experiments on the Depolarization of Light as exhibited by various Mineral, Animal, and Vegetable Bodies, with a Reference of the Phænomena to the general Principles of Polarization. By David Brewster, LL.D. F.R.S. Edin. and F.S.A. Edin. In a Letter addressed to the Right Hon. Sir Joseph Banks, Bart. K.B. P.R.S.—IV. On an ebbing and flowing Stream discovered by boring in the Harbour of Bridlington. By John Storer, M.D. Communicated by the Right Hon. Sir Joseph Banks, Bart. K.B. P.R.S.—V. On the Effects of simple Pressure in producing that Species of Crystallization which forms two oppositely polarized Images, and exhibits the complementary Colours by polarized Light. By David Brewster, LL.D. F.R.S. Edin. and F.S.A. Edin. In a Letter addressed to the Right Hon. Sir Joseph Banks, Bart. K.B. P.R.S.—VI. Experiments made with a View to ascertain the Principle on which the Action of the Heart depends, and the Relation which subsists between that Organ and the Nervous System. By A. P. Wilson Philip, Physician in Worcester. Communicated by Andrew Knight, Esq. F.R.S.—VII. Experiments to ascertain the Influence of the Spinal Marrow on the Action of the Heart in Fishes. By Mr. William Clift. Communicated by Sir Everard Home, Bart. V.P. R.S.—VIII. Some Experiments and Observations on the Colours used in Painting by the Ancients. By Sir Humphry Davy, LL.D. F.R.S.—IX. On the Laws which regulate the Polarization of Light by Reflexion from transparent Bodies. By David Brewster, LL.D. F.R.S. Edin. and F.S.A. Edin. In a Letter addressed to the Right Hon. Sir Joseph Banks, Bart. K.B. P.R.S.

Mr. Robertson Buchanan has published a new and extended edition of his valuable treatise "On the Œconomy of Fuel and Management of Heat, especially as it relates to heating and drying by means of Steam."

LXXXIV. *Pro-*

LXXXIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

June 1. THE conclusion of Mr. Donovan's paper on the berries of the mountain ash or roan-tree, the *Sorbus aucuparia* of Linnæus, was read. In the series of ingenious experiments devised by the author, on apples, pears, crabs, and roanberries, he discovered the existence of a new and peculiar acid in the latter, which he calls sorbic acid. The roanberries, although an object of vulgar superstition in all the northern countries of Europe, in which they have been deemed an antidote against witchcraft, and of a very peculiar taste, have never been submitted to any chemical analysis, since Scheele discovered in them and other vegetables the existence of malic and citric acids. Some experiments have indeed been made on malic acid by Vauquelin and Proust, but our knowledge of this and many other vegetable acids is still very imperfect. The researches and discoveries of Mr. Donovan will doubtless attract the attention of chemists to these substances. In preparing the sorbic acid Mr. D. pursued the method of Scheele as improved by Vauquelin; he expressed the juice of the ripe berries, boiled it saturated with carbonate of potash, and added acetate of lead, which was precipitated in the state of a compound salt containing malat of lead: the latter was again precipitated, and the sorbic acid obtained pure. The author related a great number of experiments on roanberries and apples, to ascertain the relative quantities of acid which they contain: he found that the sorbus berries yield nearly one-half their weight of juice, consisting of sorbic and malic acids, the former of which was much more abundant,—but that apples contain a very small proportion of sorbic acid, and that plums, pears, &c. have none. The difference between the salts formed by these acids is very striking; the sorbats of potash, soda or ammonia are crystallized salts, soluble in water but not in alcohol; the malats, on the contrary, are not crystallizable, are deliquescent, and slightly soluble in alcohol. Mr. D. also tried several experiments on other substances, and found that there is no sorbat of alumina: this fact will facilitate the complex analysis of many minerals containing alumina, which have hitherto occasioned the appearances of some anomalies which do not exist in nature. The author then proceeded to offer some original conjectures on the composition of vegetables in general, and what has been called the bitter principle in particular: the substance which communicates a bitter sensation he considers as the matrix or radical of the malic and sorbic acids. He seemed to think that these acids may

 rather

rather be chemical products than educts. The artificial existence of tannin favours his conjectures, which he proposed, however, with great diffidence, as things yet to be investigated.

A short paper by Sir Everard Home, Bart. V.P.R.S. was read, detailing some observations on the organs of respiration in lampreys and other analogous genera of fishes.

June 8. A mathematical paper by Mr. Babage was laid before the Society, On the Calculus of Functions; but its contents were of a nature not to be read.

Dr. Herschel furnished a long and very elaborate paper on the satellites of the Georgian planet. The Doctor has ascertained the existence and general laws of seven different satellites to this remote body; but he acknowledges that their extreme distance, and the numerous difficulties which arise in observing them, render him very cautious in determining any thing dogmatically on the subject. The introduction to the paper contained some interesting observations on telescopes in general, and the means best adapted to observe such distant objects. The structure and management of telescopes being the author's most familiar department, his directions are the more valuable. He observes that no glass under 20 feet is fit for viewing the satellites of the Georgian planet; that his principal observations have been made with one 25 feet; but that his great 40-feet telescope requiring so many persons to manage it, and being incapable of acting at all times with sufficient rapidity, when the light and atmosphere suit, he has very rarely succeeded in using it when viewing these satellites. He next speaks of the necessity of having sufficient light on the reflecting mirrors, which cannot always be obtained, and many other difficulties which impede the progress of our knowledge of these heavenly bodies. Lastly, he related his observations on the different satellites which he has already noticed around the planet in question, and stated the probability that some more might still be discovered as our instruments improve.

A short letter from Dr. Brewster to the President was read, stating some further experiments on the multiplying powers of Iceland spar. These powers he found to depend on the surfaces of the spar, and he can now imitate them at pleasure.

June 15. A paper on the lamprey and ëchineis, by Sir E. Home, was read. The author considered these genera as hermaphrodites, and forming a link between fishes and vermes. Sir E. stated the appearances on dissection, and also his observations on the living animals, which justified him in his conclusiens.

A curious paper by A. Carlisle, Esq, was read, on vascular and extravascular parts of organized bodies, He described the process of the formation of shells, particularly those of snails and fishes;

fishes; the manner of puncturing shells to produce pearls; and the mode which snails adopt to repair their broken shells, &c.

J. G. Children, Esq. submitted to the Society a description of his very large Galvanic battery, each plate of which consisted of 32 square feet, and related the effects of a great number of experiments made with it in producing intense heat, in melting metals, &c. One experiment was on iron. He and Mr. Pepys took a piece of soft iron, made a cavity in it to hold some diamond powder, and then submitted it to the action of the Galvanic battery; when the iron was instantly converted into blister steel, and the diamond entirely disappeared. This experiment, the author concluded, was quite satisfactory to prove that the diamond contains nothing but pure carbon.

The title of a paper by Mr. Lee, On the dispersive Power of the Atmosphere, and that of several others, were read, in order that they might be printed in the forthcoming volume of the Philosophical Transactions.—The Society then adjourned till Thursday the 9th of November.

ROYAL INSTITUTION.

Professor Brande in his fifteenth and concluding lecture presented his audience with a succinct account of the origin and progress of electro-chemical science, and dwelt particularly upon the brilliant and important discoveries and researches of his predecessor, Sir H. Davy.

The application of electricity to chemistry seems to have originated with Beccaria and Canton, and to have been brought into more general notice by the experiments of Dr. Priestley, and the refined and masterly researches of Mr. Cavendish; but nothing very important was achieved in this branch of experimental philosophy previously to the discovery of the Voltaic pile. In the earliest experiments with this instrument some of its leading chemical powers were developed, especially its decomposing energies in regard to water and saline solutions: it was also observed that the electrization of distilled water was attended with the extrication of acid and alkaline matter, a phænomenon in explanation of which a variety of crude and unsatisfactory hypotheses were indulged in: it was conceived that pure water was capable of producing acids and alkalies, by uniting with positive and negative electricity; that these bodies passed from the battery through the conducting wires into the water; and that there resulted from the decomposition of the aqueous elements, oxygen and hydrogen. The amusement of hypothesis being thus preferred to the drudgery of experiment, the advances towards truth were slow and imperfect, and philosophers seemed rather inclined to talk and reason upon the remarkable and new phænomena,

mena, than to endeavour to remove the veil of mystery in which they were enveloped, by the toilsome but sure method of experimental research,—a task happily reserved for, and ably performed by, Sir H. Davy. Mr. Brande illustrated these investigations by a series of experiments with the large Voltaic apparatus employed in his former lecture, and, having summed up the leading conclusions, enumerated the discoveries of which they had been productive. It was proved that the acid and alkaline matter was derived from the presence of foreign bodies; that the elements of water and of the atmosphere had given rise to nitric acid and ammonia; and that, every extraneous body being carefully excluded, water was resolved by the electrical energy into oxygen and hydrogen only. Having taken a cursory view of electro-chemical theory in general, and having reminded his hearers of the discovery of the nature of alkaline and earthy bodies,—a discovery resulting entirely from these researches,—the Professor concluded his lecture with some general observations concerning the nature of electricity, illustrated by several new and interesting experiments: he combated the idea of its being a peculiar fluid, and of its phænomena resulting from the presence of any distinct subtance or form of matter: it might be so, but we must not say it *is* so, till it be proved.

LXXXV. *Intelligence and Miscellaneous Articles.*

We are informed that the extensive and valuable collection of minerals of the Rev. Richard Hennah, late of St. Austell in Cornwall, and which is now in the possession of his son the Rev. R. Hennah of Plymouth, consisting of nearly 2000 specimens of the most rare and curious productions of that county, particularly of tins, is to be disposed of.

NEW STEAM-BOAT.

In addition to the steam-boat now plying between London and Gravesend, a very fine vessel of this description has recently commenced sailing between London and Margate. She is named the Thames, was built at Port Glasgow from the plans furnished by Mr. Robertson Buchanan, engineer, and is the same vessel the drawings of which, with a description by Mr. Buchanan, were given in a preceding number of the Philosophical Magazine. The Thames previous to her arrival in the river had been navigated from the Clyde to Ireland across the Irish channel, by the Land's End, and round to Portsmouth and the Downs, a voyage of 1500 miles, in perfect safety.

METEORO-

METEOROLOGICAL TABLE,

BY MR. CARY, OF THE STRAND,

For June 1815.

Days of Month.		Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
May	27	60	66	54	30·05	61	Fair
	28	60	68	55	29·95	62	Fair
	29	57	56	54	·90	0	Rain
	30	57	66	55	·88	56	Fair
	31	56	66	45	·86	62	Fair
June	1	46	58	58	30·08	42	Cloudy
	2	60	69	60	·04	46	Showery
	3	59	63	64	29·95	51	Fair
	4	62	69	57	·85	56	Fair
	5	60	68	57	·58	58	Showery
	6	56	62	54	·48	36	Showery
	7	56	67	53	·56	49	Fair
	8	57	69	58	·64	56	Cloudy
	9	58	70	55	·80	61	Fair
	10	56	71	60	·87	85	Fair
	11	60	74	60	·84	80	Fair
	12	55	60	52	·68	57	Stormy
	13	56	62	55	·52	46	Stormy
	14	57	66	57	·33	36	Showery
	15	59	64	53	·76	56	Fair
	16	60	72	60	·80	60	Fair
	17	61	73	60	·60	66	Fair
	18	60	70	61	·62	60	Cloudy
	19	61	69	63	·71	66	Fair
	20	64	72	60	·76	60	Fair
	21	62	70	59	·81	61	Showery
	22	60	67	55	·84	45	Showery
	23	54	67	58	·99	57	Fair
	24	57	69	57	30·06	59	Fair
	25	55	66	51	29·99	46	Cloudy
	26	54	69	54	30·10	52	Fair

N.B. The Barometer's height is taken at one o'clock.

ERRATA.

P. 338, running title, *for* geographical *read* geological.
340, line 11, dele (the algebraic sign).
337 & 8, l. 19 & 20, *for* experimental *read* exponential.

INDEX TO VOL. XLV.

END OF THE FORTY-FIFTH VOLUME.

Printed by Richard and Arthur Taylor, Shoe Lane, London.

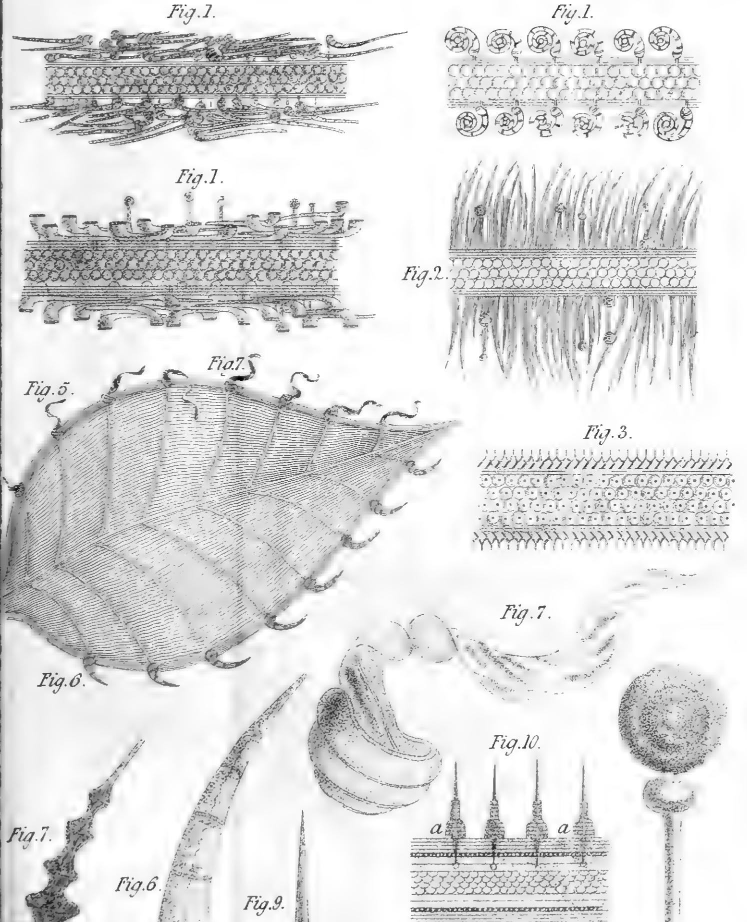


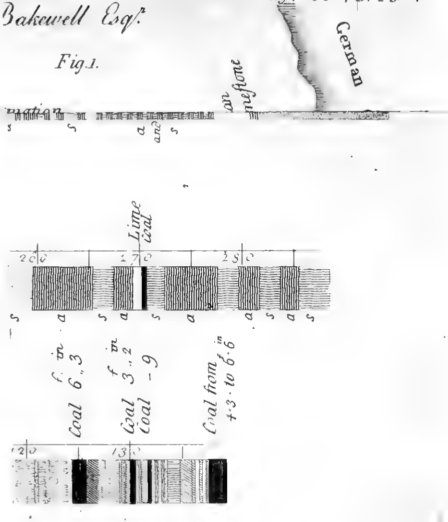
Fig. 1.
Fig. 1.
Fig. 1.
Fig. 2.
Fig. 7.
Fig. 5.
Fig. 6.
Fig. 3.
Fig. 7.
Fig. 10.
a
a
Fig. 7.
Fig. 6.
Fig. 9.
Fig. 8.
Fig. 12.

S. Porter sc.

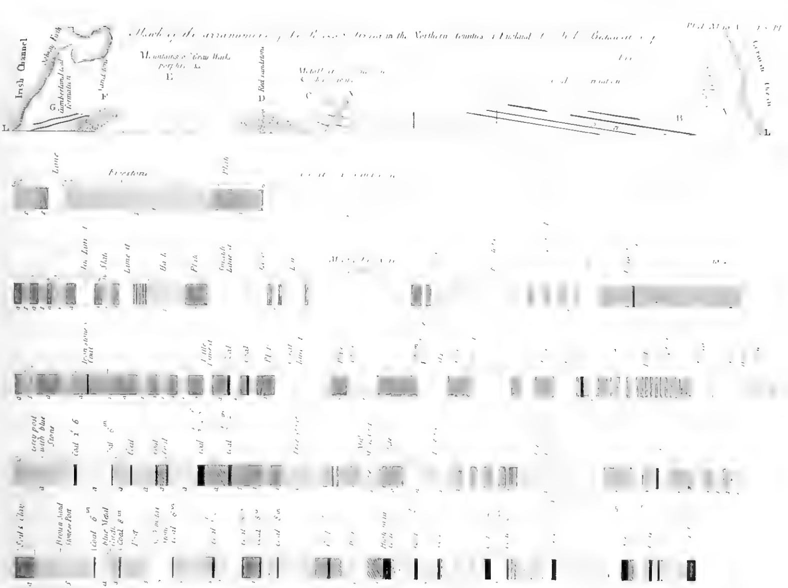
Phil. Mag. Vol. 45. Pl. 2.

Bakewell Esqr.

Fig. 1.



S. Porter sc.


Irish Channel
Solway Firth
Cumberland Coal formation
G
F
Mountains of Cross Fell
E
Red sandstone
D
C
B
L
Freestone
Lime
Plate
Grey post with blue Stone
Coal 1' 6
Coal
Brown Sand Stone Post
Coal 6"
Blue Marl
Coal
Post
Soil & Clay

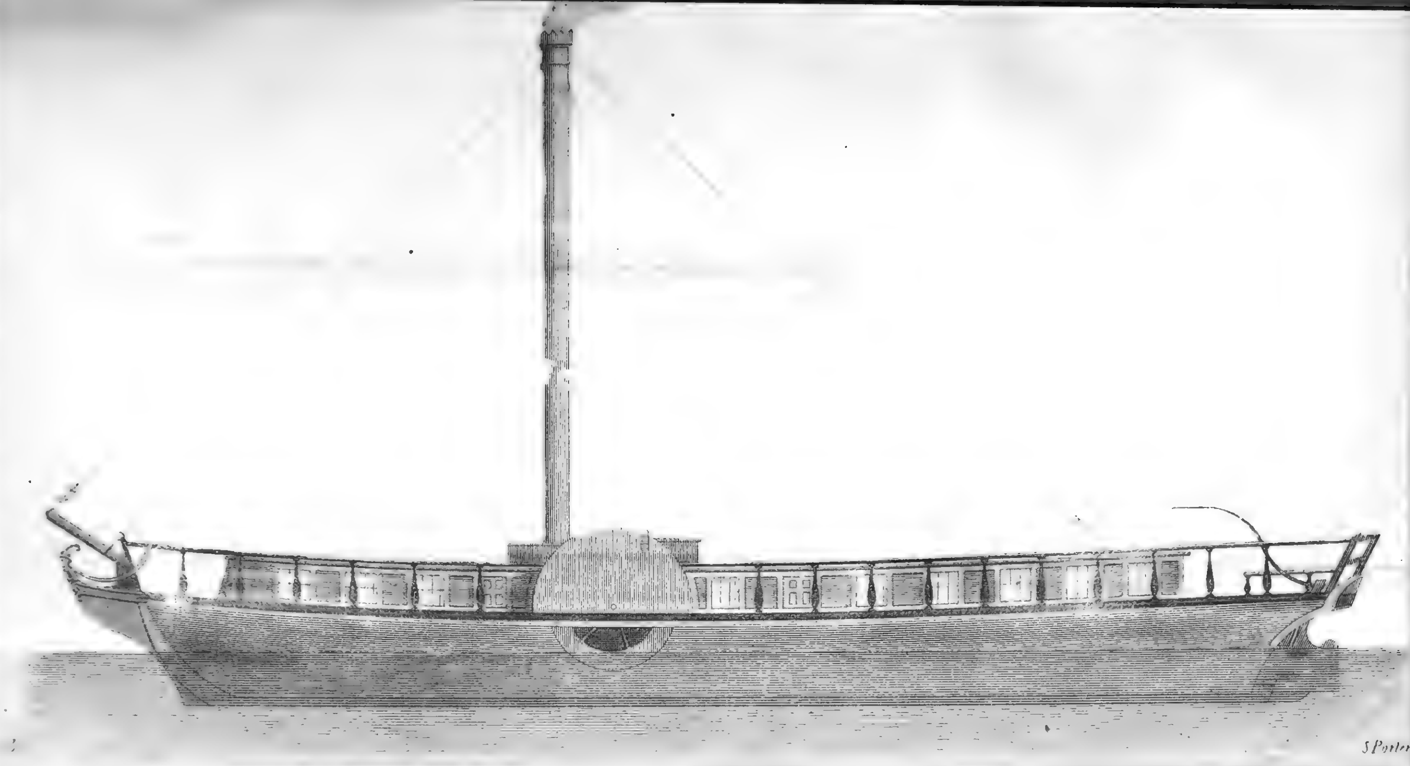

S Porter

Fig 1

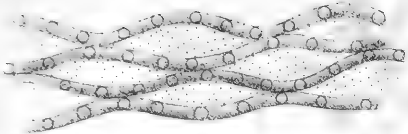
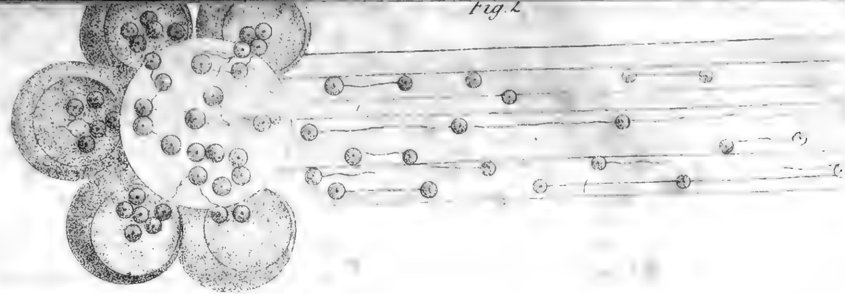
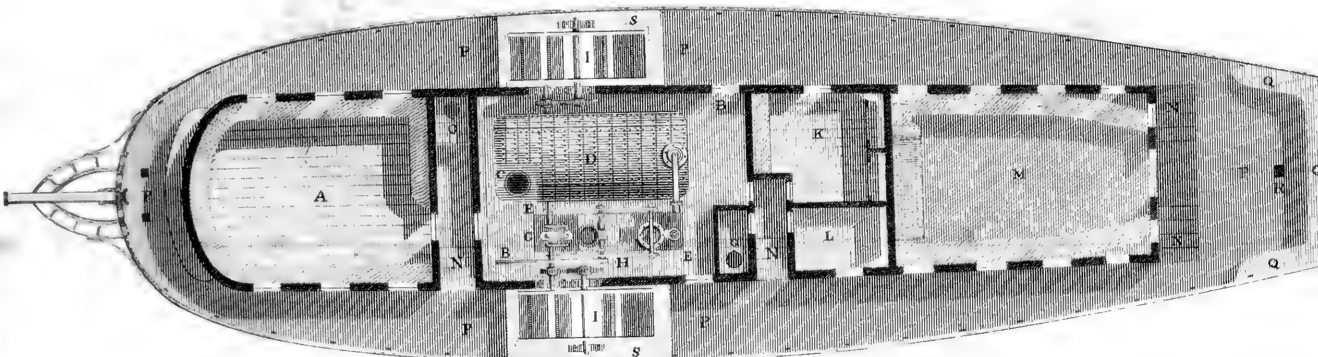
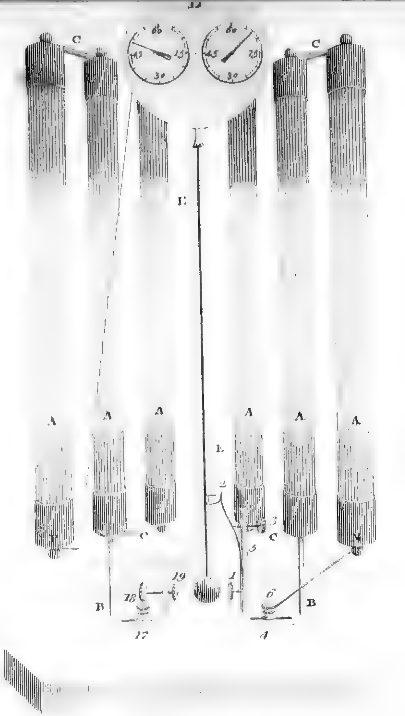


Fig 2





S. Porter sc.



204

S Porter sc

Fig. 1.

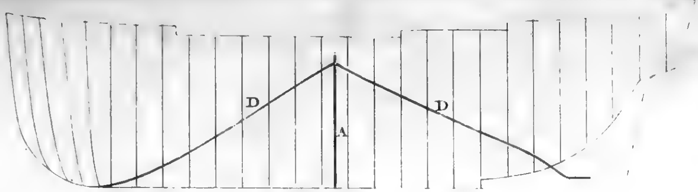


Fig 2

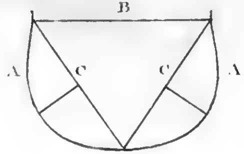
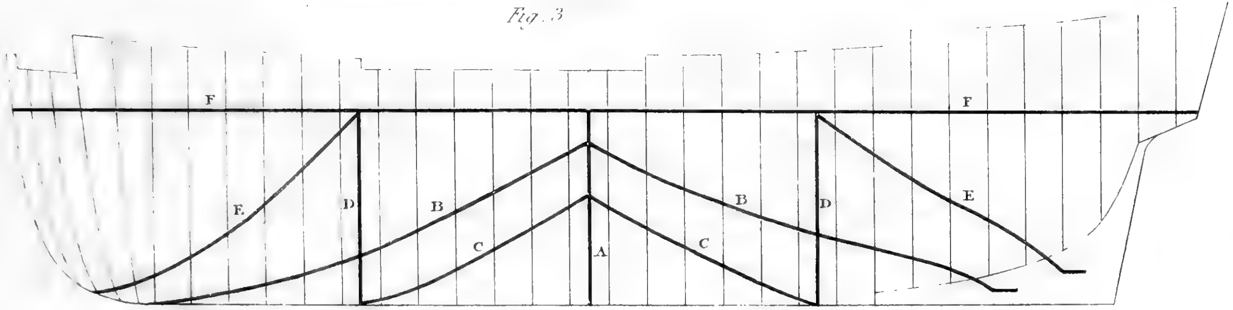


Fig. 3



204.

S. Porter. sc.

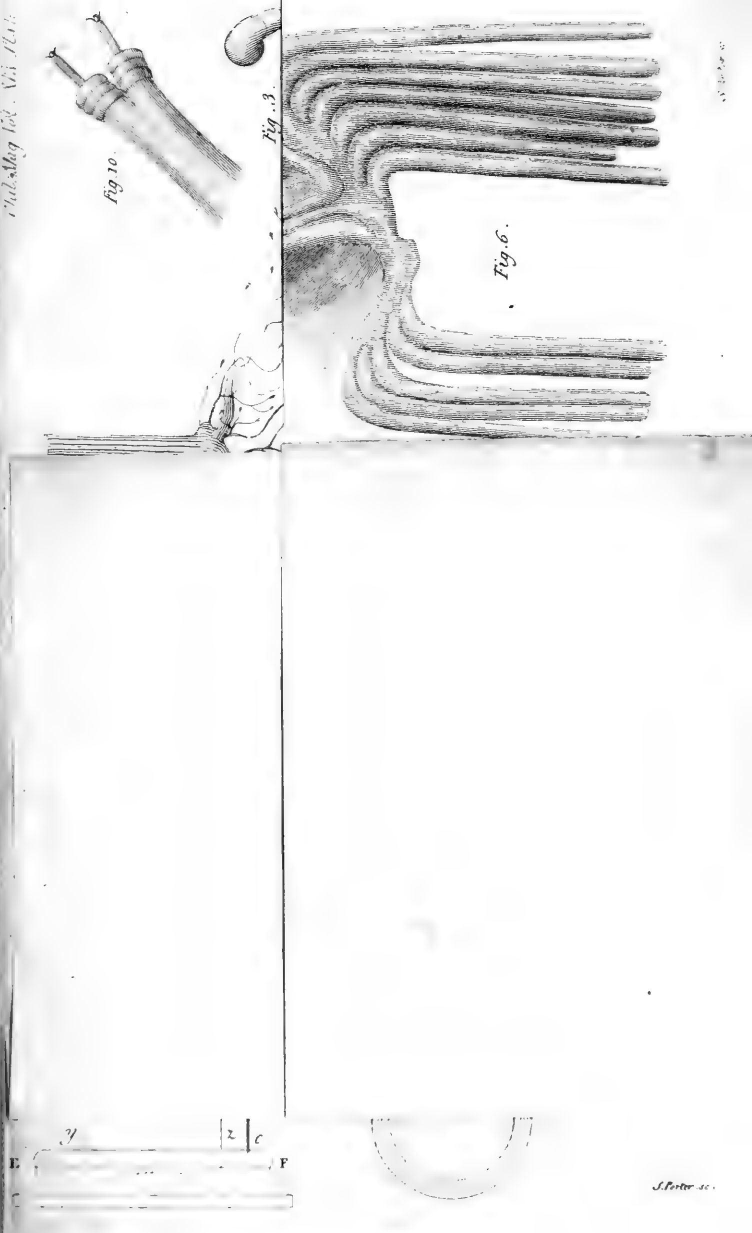
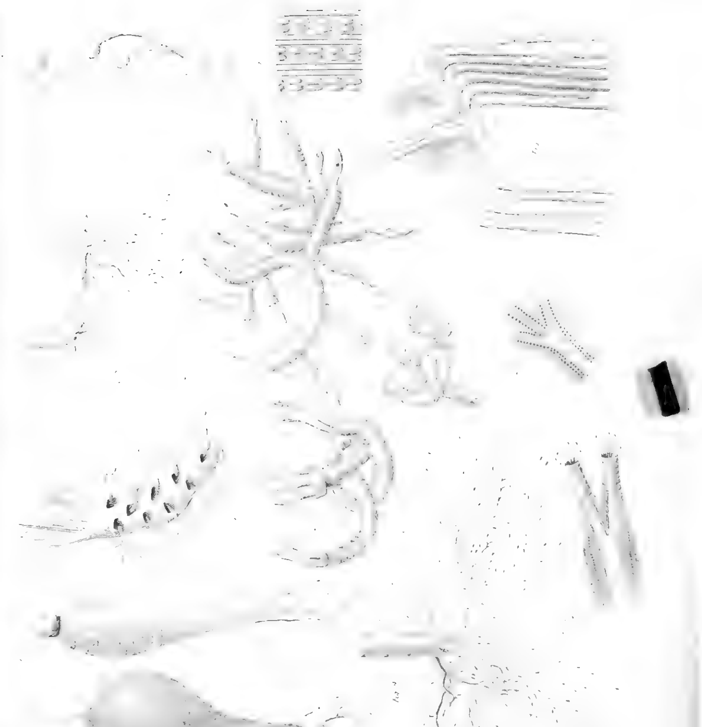
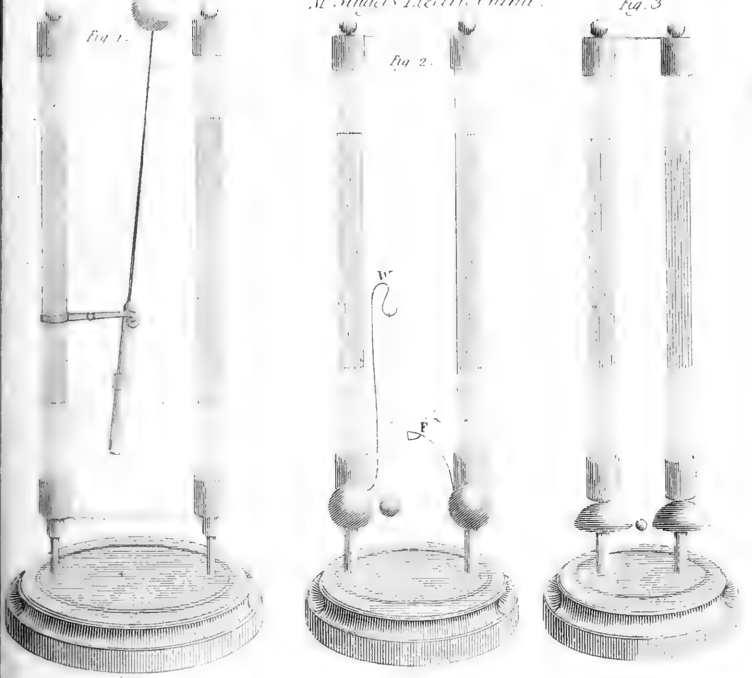


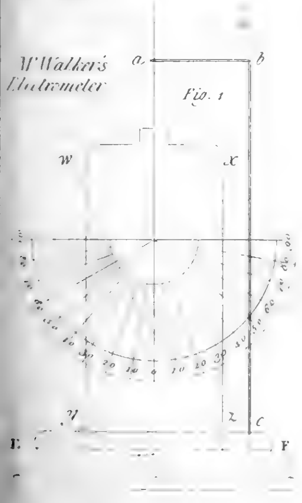
Fig. 10.
Fig. 3.
Fig. 6.
y
z
c
E
F
S. Porter sc.



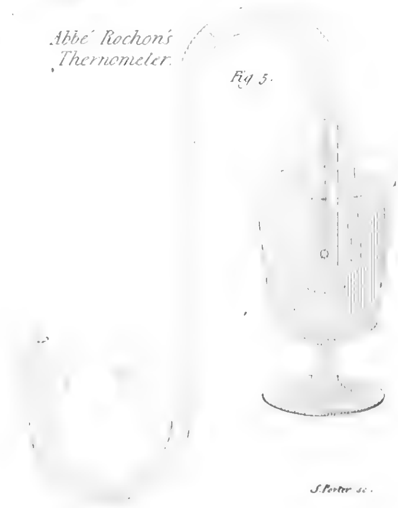
Mr Singer's Electric Chime.



Mr Walker's Electrometer



Abbé Rochon's Thermometer.



J. Porter sc.